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PHYSICAL FACTS AND SCRIPTURAL RECORD;

OR,

Eighteen Propositions for Geologists.

BY

W. B. GALLOWAY, M.A.

VICAR OF ST. MARK'S, REGENT'S PARK,
AND CHAPLAIN TO THE RIGHT HON. LORD VISCOUNT HAWARDEN.

"Thy word is tried to the uttermost, and Thy servant loveth it."—Ps. 119.

RIVINGTONS,
London, Oxford, and Cambridge.

1872. 20

TO THE RIGHT REVEREND
THE LORD BISHOP OF ELY.

MY DEAR LORD,

I ESTEEM it a high privilege to be permitted to inscribe these pages to one who, amid the occupations of an exalted and laborious office, has found leisure to stand nobly forward in the lead; to meet and answer the insidious attacks upon the Christian faith which have been so unsparingly made; and from whom the labour expended and the results obtained by others in this good cause do not pass without frank and generous appreciation. But it is with extreme diffidence that I lay before you a work in a department such as the present. It is necessary to encounter Infidel Science—no slight antagonist, but an adversary who now vaunts himself in the mature and magnificent development of his stature, and who has arrayed his ponderous frame with the most polished and massive scientific armour, as an altogether impenetrable panoply. I am conscious that the light equipment with which I enter the field may provoke confident disdain; but I enter it with a humble trust in the God of truth, the Creator and Redeemer of the world, and with a steady eye for the joints of the harness.

I venture to think that a direct and unexpected evidence of the historical truth of an interesting and important part of the Word of God will be found conclusively furnished from the facts revealed in His own works of nature. In applying myself to this investigation amid the constant engagements of parochial duty, I have not been unmindful of the vows of God which are upon me, and of the promise to be diligent in such studies as help to the knowledge of the Holy Scriptures. In the interest of truth I humbly invite, rather than deprecate, strict scrutiny, trusting that the imperfections which, I am conscious, have attended the execution of the work will be forgiven, if the main positions which I have endeavoured to establish shall prove to be secure.

To God be the praise if any thing worthy of the good cause shall be found accomplished. In the present state of religion, betrayed by some of its sworn defenders, and while the land is filled with unbelief, not only in its scientific circles, but in many of the lowest of its workmen, who propose to renovate society on principles similar to those of the enlightened atheistic savans, which bore memorable fruit in 1793, it may suffice to recall the subdued yet devoted sentiment of an ancient soldier amid the disasters of his country, "*Quicquid fecerimus, satis grande est.*"

With a warm sentiment of reverential and dutiful regard,

I am, my dear Lord,

Your Lordship's faithful and grateful Servant,

W. B. GALLOWAY.

Nov. 17, 1871.

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ERRATUM.

Page 165, line 25, *for* diamagnetic *read* paramagnetic.

PROPOSITION I.

IT is a well-known law of the solar system that the secondary planets, or moons, revolve about their axes in the same time in which they revolve round their primary planets.

Hence, in conjunction with other circumstances, it has been conjectured by some, and among the principal by Laplace, that the matter now consolidated in the secondary planet had at one time been part of a more expanded and attenuated condition of the primary, which had filled the whole space of the orbit of the secondary, and which, according to a law similar to that which is familiarly illustrated in the different times of oscillation of pendulums of different lengths, would then revolve on its axis very much slower than now, in proportion to the immensely greater length of semidiameter. For, other things being equal, the rotatory velocity of the earth depends on its volume. So that the moon's revolution on its axis might be considered a modified remnant of the same original fact with its revolution in its orbit; namely, of its having been part of the earth itself in a previous uncondensed condition of the planet. It is also not improbable that the condensation from that state did not take place all from the centre outwards, but towards various points as centres of many separate

condensations, which may afterwards have been conglomerated towards one primary centre, and perhaps one or more secondary.

Whatever may be thought of the conjecture of the eminent astronomer who has been mentioned, there is no doubt of the facts on which it was grounded, whether they are to be explained on that hypothesis, or by some other. (See Appendix, A.)

PROPOSITION II.

IN harmony with the preceding proposition, the orbits of the secondary planets or satellites are found to be generally in or near the plane of the equators of their respective primaries, that is, each in the direction of the rotation of its own primary planet, and in or near the central plane to which the axis of that rotation is perpendicular.

This is remarkably illustrated in the moons of Jupiter and Saturn. The former planet moves in an orbit inclined only $1^{\circ} 19'$ to our ecliptic, and within $3^{\circ} 4'$ of coinciding with the plane of his own equator; and with these the orbits of his four satellites are so nearly in agreement, that they are eclipsed every time they pass, except the fourth, which, from its greater distance, sometimes escapes. The plane of the orbit of the first satellite very nearly coincides with the plane of Jupiter's equator. The inclination of the orbit of the second to the plane of the planet's equator is only $27' 49''\cdot 2$. The orbit of the third is inclined to the equator of Jupiter $12' 20''$. The orbit of the fourth, in like manner, is very little inclined to the equator of his primary, though, I believe, slightly more than that of the others. Again, the orbits of all Saturn's satellites, except the fifth, are nearly in one plane. At the time of their greatest digressions they are always situated in the same straight line

with the major axis of his ring. And at those times, when Saturn is so turned as to present his ring edgeways to us, his satellites sometimes appear like beads threaded on the thin line of the ring. Herschel discovered, by the help of bright spots in the ring, that it turns round its axis, and that this axis is also that of Saturn's rotation. The ring itself is now thought not to be of a rigid structure, but to consist of a vast multitude of small bodies moving as satellites, like those rings of innumerable meteoric bodies now known to be revolving round the sun or earth, and whose orbits seem to intersect the orbit of the earth. Saturn's rings, therefore, in that case may be taken, with his seven satellites, as affording a multitudinous exemplification of this general rule of the secondary planets. And so much is this rule recognized that the position of the axis of Uranus is only inferred from it, as being probably not far from perpendicular to the plane of the orbits of his satellites, and therefore within eleven degrees of the plane of his own orbit round the sun. For this inference proceeds on the assumption of the law that the satellites revolve approximately in the plane of the planet's equator.

PROPOSITION III.

BY a marked divergence from the preceding rule the orbit of the moon is now inclined to the plane of the earth's equator at a minimum angle of about $18\frac{1}{2}$ degrees; being inclined only $5^{\circ} 9'$ to the plane of the ecliptic, to which the earth's equator is now inclined $23^{\circ} 27' 23''$, but at the date of the eclipse of Agathocles, B.C. 310, it is calculated by the Astronomer Royal, in two papers of which he did me the honour to present me with a copy¹, to have been inclined $23^{\circ} 43' 42''$, and at the time of the eclipse of Thales, B.C. 585, the obliquity was $23^{\circ} 45' 55''$. Either, therefore, since the time when the moon revolved in the plane of the earth's equator, that plane has been tilted, by a change in the earth's axis of diurnal rotation, to the extent of about eighteen degrees and a half, or else the change has taken place in the orbit of the moon by some external force; which latter is less likely, especially as the orbit of the moon deviates so much less from the plane of the ecliptic in which the earth's orbit lies.

Probably by the same event, whatever it was, the moon's equator was tilted in relation to us to the extent of $6^{\circ} 39'$, being inclined $1^{\circ} 30'$ to the ecliptic on the other side. For as the crossing of the

¹ Published in the "Philosophical Transactions" of 1853 and 1857.

moon's orbit and her equator always coincides with the crossing of her orbit and the orbit of the earth (the ecliptic), that inclined position of her axis is indicated to be connected with her relation to the earth; and it would probably result from the tilting of the protuberant matter of the earth's equator acting on the moon's somewhat prolate form towards the earth. But the smaller degree of the inclination of the moon's equator relatively to her orbit ($6^{\circ} 39'$), being little more than a third part of the tilting of the earth's equator ($18\frac{1}{2}^{\circ}$) relatively to the earth's own former position as indicated in the inclination of the moon's orbit to the ecliptic ($5^{\circ} 9'$), shews that the cause, whatever it was, acted primarily on the earth, and only through the earth, by remoter consequence, upon the moon.

It appears to me, on a comparison of the circumstances, that the event was sudden; and our consideration of it may be simplified by assuming it to have taken place when the moon was in the node. If the change had been gradually going on from the beginning, in the course of illimitable ages, the moon would more probably have otherwise adapted itself, being drawn gradually by its relation to the earth into the same deviation with it from the original plane of its revolution; and this may seem to have happened in the case of the moons of Saturn, if the equator of that planet was formerly nearer than now to the plane of its orbit.

PROPOSITION IV.

A CHANGE or modification of the earth's balance, such as would account for the divergence above named, might be occasioned either by some violent internal convulsion—a supposition manifestly not beyond what has been entertained by astronomers, since it has even been thought by some of them that the small planets called asteroids, upwards of ninety in number, between the orbits of Mars and Jupiter, are fragments of a large planet which had exploded and blown to pieces; or, which may seem more probable, it might be occasioned by some considerable accession of matter from without, such as some part of a revolving ring of planetary matter in space falling on to the earth, and occasioning subsequent superficial and internal disturbances; by the combined result of which the axis of rotation might be considerably affected.

The weight of the opinions of Newton and of Laplace is cited by Sir Charles Lyell against the admission of such a change of axis; nevertheless the astronomical facts stated in the preceding propositions may point in that direction. Some things, also, which have come to light since the time even of Laplace might have modified the opinions of these great men; and, notwithstanding their high names, the thought is not rejected by all who are reputed

men of science. The late eminent Professor Robison thought it "by no means impossible, nor highly improbable." Sir John Lubbock inclines to it. "The possibility of such a change," says he, "has been denied by many astronomers. My father, the late Sir J. W. Lubbock, on the contrary, has maintained (Geol. Journ., vol. v. p. 4) that it would necessarily follow from upheavals and depressions of the earth's surface, if only they were of sufficient magnitude. The same view has recently been taken by other mathematicians¹." Mr. Croll, who is cited with approval by Sir Charles Lyell, supposes that even the accumulation of ice at the pole might be such as to affect the earth's centre of gravity²; and, while Sir Charles himself in more than one place contemptuously rejects the supposition of a change of axis, he nevertheless confesses that some geological facts seem to indicate the polar circle to have been more limited, and the inclination of the equator to have been at one time less by several degrees, for the obtaining of which he has no scientific grounds; but, transgressing the calculated limits of the variation in the inclination of the equator to the ecliptic, he betakes himself to his usual easy resource, not in this instance paltry hundreds of thousands, which would confessedly do nothing, but *millions* of years³. But that indefinite and purely hypothetical resource might be unnecessary, if evidence were found that the earth was tapped on its shoulder from without.

¹ Sir J. Lubbock, "Pre-historic Times," p. 392.

² Sir C. Lyell, "Principles of Geology," i. 272.

³ Ibid. 282.

PROPOSITION V.

THE existence of a ring of diffused planetary matter, or planetary fragments, whose orbit, at least partially, from time to time intersects or impinges upon the orbit of the earth, is proved by that periodical return of the maximum showers of the November meteors every thirty-three or thirty-four years, which was remarked and was predicted by Olbers¹; a prediction which was conspicuously verified in the years 1866 and 1867; while a still greater and denser fall is calculated as probable to occur at the close of this century, as Mr. Denison, on the authority, I believe, of Professor Newton in America, and Mr. Alexander Herschel in this country, has informed us².

According to Professor Encke, the point in planetary space from which the meteors on former visits issued, and particularly as observed in 1834, was the goal of the earth's motion at the time³; and in subsequent displays also it has been observed that they came from that point in space towards which the earth was then moving.

¹ Humboldt, "Cosmos," i. p. 115.

² "Astronomy without Mathematics," p. 200.

³ "Introduction to Meteorology," by David Purdie Thomson, M.D., Edin. 1849.

But the matter which in past ages existed in that vast ring must then have been denser, and the showers thence at one period more severe, considering that the continual falling of meteors thence for some hundred recurrences of the shower has abstracted much from it of what formerly was in the heavens. The evidences of some prodigious meteoric falls of enormous weight and extent have been misunderstood, and ascribed to other and even impossible causes.

Now, if on a theory such as Laplace's, the secondary planets were originally a portion of the primary in an expanded state, and if, rotating with the primary planet, there first was formed a ring, dilated, and more and more dilating from the centrifugal force, resembling those of Saturn, and which, by and by parting, whether in the progress of dilation, or of condensation at various centres, had its matter conglomerated principally into one or more globes, moving as a moon or moons around the primary planet, then it accords with reason that many other smaller portions of the same ring, not thus conglomerated together into one, would, in a scattered manner, form smaller rotating bodies of a similar nature, and would assume similar, or possibly in some cases more eccentric, orbits. These smaller bodies, if supposed existing in the system of our earth and moon, being acted on by the attraction of both, would occasionally fall in showers either on to the earth, or into the moon, as circumstances, connected probably with the revolution of the apsides of their respective orbits, as well as with the nodes, brought them within the predominating attraction of either, but principally of the earth. Their fall also would probably occur when the returning path

of their orbit relatively to the earth⁴, being drawn somewhat inward by the relative position of the moon and of the sun, met the onward path of the earth, so that they would come from the direction towards which the earth was moving, and, as it were, with the velocity of the earth meeting that of the moon⁵, and would chiefly fall towards the shoulder of the earth which was averted from the sun; by night, therefore, and not by day. Nevertheless the stones projected from them pursuing their onward parabolic course might make a considerable circuit round the earth, and come down by day. This might be expected to occur within the earlier periods after the moon was set in her orbit, such as it would never again in equal quantity and manner in later ages; so that modern meteoric showers may be but thin remnants, and sporadic droppings, of what once were awful and portentous events; for stones, once fallen, are not again exhaled into the atmosphere, like water, to fall anew. In the revolution of the varying lines of the apsides of the several fragmental currents, it would not be surprising if three revolutions of them should pass clear, but in the course of

⁴ It may be remarked that the path of these, like that of the moon, would nowhere assume a retrograde direction relatively to the *sun*, but, though circling in an orbit round the earth, would always be onward as regards the sun; as a nail on a carriage-wheel, though revolving round the axle, is at no point of its revolution retrograde as regards the road, which, for our present purpose of illustration, may be regarded as part of an orbit round the earth's centre.

⁵ The average velocity of the earth is about 18·2 miles in a second, and in November it would be somewhat more. The velocity of the moon is rather more than thirty-eight miles in a *minute*. But the velocity of the meteors in 1866 was calculated to be about double that of the earth; and if so, their orbit must be assumed to be much nearer to the earth than the moon is, and to be more eccentric, giving a much greater velocity at the perigee.

the fourth a portion should be drawn fully within the attraction of the earth. For it is to be considered that not only the satellite ring or stream of meteors must be supposed at their nearest distance to the earth, but also that that nearest distance should concur with the line of the sun and moon's attraction, in such manner that these heavenly bodies should strengthen the attraction towards the earth. Measuring by the revolution of the moon's apsides, of which four revolutions occupy 35 years and $146\frac{1}{2}$ days, a cycle of 33 years in the showers might not be an improbable interval for some of the currents, especially since 33 years correspond nearly to 34 lunar years, or synodical years of the sun and moon's nodes. In some other cases it may be thought that the currents might be not scattered portions of satellite rings, but strictly planetary; though the intersection of a planetary orbit with that of the earth, especially having the movement in the opposite direction to the earth, and at nearly the same distance from the sun, appears extremely improbable, if not impossible, and the correspondence of the lunar periods appears not without signification.

Now without attempting thus to define the process of that creative work, of which the rings of Saturn have been thought by some to furnish a suggestive illustration, it is clear that to those at least who attach probability to that scientific hypothesis which has been referred to (and they are, it is believed, the majority of astronomers), the supposition of vast showers of the scattered elements of such a parted ring having fallen upon the earth, and given rise to considerable deposits within the early ages of this habitable globe, could not consistently be thought to present any reasonable difficulty.

PROPOSITION VI.

THE magnetic polarity of the earth has some relation to the earth's axis of rotation; and it is in consequence of this that it has served, in the mariner's compass, for a guide upon the ocean. Nevertheless, the north magnetic pole may be estimated to be about $18\frac{1}{2}$ degrees distant from the north pole of the earth's present axis of rotation, or about the number of degrees indicated in the obliquity of the earth's equator to the plane of the ecliptic, *minus* the inclination of the moon's orbit to the ecliptic. So that, if the axis of the earth's rotation was at one time perpendicular to the moon's orbit, and the north magnetic pole coincided with the pole of the earth's axis, the change of the axis, in relation to the old plane of the equator and the moon's orbit, would place the *old* pole and the *new* at the actual distance of the magnetic pole and the pole of the earth's rotation.

PROPOSITION VII.

AS all terrestrial substances, according to the beautiful discovery of Faraday, are either *paramagnetic*, that is, arranging themselves in the direction of the magnet, or *diamagnetic*, that is, arranging themselves transversely, at right angles to the magnet, when subjected to the same magnetic influence; it seems inevitable that this fact must have had some influence in the structural arrangement of the parts of the earth's mass and crust, relatively to the earth's general magnetism, in the process of their solidification.

The diurnal revolution of the earth may have exercised some influence of the same kind in regard to the direction of strata: and a change of the earth's pole might leave its mark in the direction of some of the leading lines, and also in the magnetism. It is well known that a solid bar of soft iron or steel will acquire magnetism by position in the magnetic dip, and when acquired will retain it, though its position be changed; and the same may be expected in regard to the solid strata of the earth. But by heat the power of a magnet is weakened, and when it arrives at that degree called a white heat, it is entirely destroyed. Many causes may have disturbed or destroyed many of the traces of the original law of structure, yet some may remain in the strata and

configuration of countries. The following special instance may contribute to illustrate the present proposition; but I believe that a much more extended application may be allowed as probable.

Speaking of the slate rocks in a portion of Wales, more than thirty miles in length and eight or ten in breadth, Professor Sedgwick observes, "Crystalline forces have rearranged whole masses of them, producing a beautiful crystalline cleavage passing alike through all the strata. And again, through all this region, whatever be the contortions of the rocks, the planes of cleavage pass on generally without deviation, running in parallel lines from one end to the other, and inclining, at a great angle, to a point only a few degrees west of the magnetic north. Without considering the crystalline flakes along the planes of cleavage, which prove that crystalline action has modified the whole mass, we may affirm that no retreat of parts, no contraction of dimensions, in passing to a solid state, can explain phenomena such as these. They appear to me only resolvable on the supposition that crystalline or polar forces acted on the whole mass simultaneously in given directions, and with adequate power¹."

¹ Quoted in "Milner's Gallery of Nature," p. 631.

PROPOSITION VIII.

THE magnetism of the earth has been found to be notably influenced by the sun and moon, which are also the principal bodies influencing the earth by gravitation, and ministering to it light.

This magnetic influence of the sun and moon has been ascertained in the constant ordinary phenomena of terrestrial magnetism as regards its diurnal and annual variations. The direction of the semi-annual inequality is the same in the northern and southern hemispheres; the north end of the magnet pointing more to the east both at Kew and at Hobart Town when the sun is north of the equator, and more towards the west when the sun is south of the equator¹. Whence it appears that it is not the effect of temperature, nor influenced by it. Also, the influence of the sun has been observed in what are called magnetic disturbances and storms. On one remarkable occasion, a magnetic disturbance appeared to be the effect of a luminous spot of intense brightness, which was observed to pass over the sun's disk in September, 1859. The singular phenomena which presented themselves, and which have been attributed to that

¹ Major-General Sabine on the "Results of the Magnetic Observations at Kew Observatory from 1857 and 1858 to 1862, inclusive."—*Philos. Trans.*, 1863, p. 274.

luminous spot in the sun, are related by Sir J. Herschel in his "Familiar Lectures on Scientific Subjects" (p. 80), and are referred to by Sir Charles Lyell in his "Principles of Geology" (vol. ii. p. 231). The regular diurnal and annual variations of terrestrial magnetism as influenced by the sun and moon, as well as the secular variation, we shall have occasion to refer to afterwards, and may confine ourselves here to the above general statement of a proposition which is undeniable, and which is sufficient at the present stage of the argument.

Thus, while magnetism has been almost, if we may not say, as some think, entirely, identified with electricity, which is known to have close relations to light, and also to molecular attraction, terrestrial magnetism bears a marked relation to the sun, the centre of light and heat, which has, at the same time, within its sphere the centre of attraction in the solar system. The illustrious Galileo seems to have had a presentiment of the discovery of a law of gravitation in the solar system, and a disposition to refer it to the magnetic power. And Milton, who had personally known and visited Galileo in the prison of the Inquisition, and by whom philosophical thought was "married to immortal verse," says of the planetary orbs revolving round the sun,—

"They, as they move
Their starry dance in numbers that compute
Days, months, and years, towards his all-cheering lamp
Turn swift their various motions, or are turn'd
By his magnetic beam, that gently warms
The Universe, and to each inward part
With gentle penetration, though unseen,
Shoots invisible virtue even to the deep;
So wondrously was set his station bright."

And in another place,—

“ What if the sun
Be centre to the world, and other stars,
By his attractive virtue and their own
Incited, dance about him various rounds ? ” &c.

He puts also the properties of light in reference to polarization in comparison, as it were, with positive and negative electricity:

“ And other suns perhaps,
With their attendant moons, thou wilt descry
Communicating male and female light,
Which two great sexes animate the world;
Stored in each orb perhaps with some that live*.”

It was the opinion of my esteemed friend, the late Mr. Edward Highton, well conversant with electric science, and distinguished as the inventor of one of the electric telegraphs, that the law of gravitation, and the rotatory and orbital movements of the planets, would ultimately be found to be the result of the paramagnetic and diamagnetic properties of bodies, which would tend to produce rotatory movement and revolution in an orbit. The analogy between those rotatory movements artificially produced by magnetism in Professor Faraday's experiments, and the revolution of the planets in space, is, however, not very close, the conditions being so different. Again, the rotatory movement of a common projectile requires no magnetism to explain it. Yet the centripetal force acting on the planets may, not improbably, be eventually identified with the magnetic, and there is a certain modified uniformity of character and direction, even about the rotatory movement of the several bodies in the solar system, which may indicate its original connexion with a common physical cause

* “ Paradise Lost,” b. iii. 579 ; b. viii. 122, 148.

such as this. Boschovich had long ago constructed a theory³ upon the fact that, as gravitation, according to Newton, is the attraction of every particle of matter for every other particle, that general law must stand more closely connected with the corpuscular constitution of bodies, and with the laws of corpuscular attraction and repulsion, than had been generally supposed. His method of treating the subject is abstruse, hypothetical, and mathematical, rather than practical and experimental. But in the constitution of bodies how great a sway may be assigned to electricity, the recent progress of science has tended to illustrate. Professor Faraday also, while he failed to establish the fact by experiment, says that, though the results he had obtained were negative, they did not shake his strong feeling of the existence of a relation between gravity and electricity⁴.

If gravitation itself were to be thus resolved into a result of the laws of magnetic force, the special phenomena of terrestrial magnetism, as distinguished from weight, would be due to the special direction given to a portion of the power in the solid strata and fluid parts. But, apart from that question, the known cosmical relations of the facts seem to me to suggest a probability that the terrestrial magnetic forces would at first place themselves mainly with reference to the position of the axis of rotation and plane of revolution⁵, and would to a considerable extent regulate the

³ "Theoria Philosophiæ Naturalis redacta ad unicum legem," Venet. 1773.

⁴ Faraday, "Experimental Researches in Electricity," 24th Series; Bakerian Lecture, read Nov. 1850.—Philos. Trans.

⁵ "If there were an external cause for magnetism," says Sir G. B. Airy, "it seems scarcely conceivable that some large part

general position of much of the solid strata axially, or transversely and equatorially, and, subordinately to these, at some crystalline angle to those positions; so that the deviation of the earth's magnetism from the earth's axial and equatorial position may, if other facts be in agreement, be indicative of a change of the earth's axis having occurred; and, if such shall be proved to have taken place, the irregularity of the position of the earth's leading lines of strata and mountain chains may partly be the effect of disturbances and disruptions resulting from that change, yet may, in their direction, still carry some suggestion of it. The British isles, for example, with Italy, Greece, the Adriatic, and the Red Sea, have so marked an angular position from the north-west to the south-east, that we should not be displeased to discover some common cause which had shifted them from a more symmetrical position originally held in or near the axial and equatorial directions; and a cause which would directly account for a considerable portion of their obliquity, and which would necessarily have called into action violent subterranean and also tangential or centrifugal forces, such as might afford reasons for the rest of the deviation, or, at least, for the greater part of it, may not be undeserving of consideration. But these things are here mentioned merely as indicating how this general proposition may bear upon some remoter facts and inquiries, not with the intention of at present assuming any thing concerning them.

of it should not act in planes parallel to the geographical equator" ("Treatise on Magnetism," p. 100). If the cause be one common to the external cosmical bodies, and, with nearer and therefore stronger force to the earth itself, there seems no reason why the proposition should not still hold in the qualified manner above stated, while its deviation is at the same time accounted for.

PROPOSITION IX.

IN revolution in an elliptic orbit round the sun, when the revolving body consists of solid and fluid parts of unequal density, as in the case of the earth, and is acted on by the attraction of the sun and moon, not only will there be tides in the course of the diurnal rotation, but, when the poles are set obliquely to the plane of the orbit, the eccentricity of the orbit will make its effect perceptible in drawing the water, or other movable fluid, in a predominating degree towards that pole which is turned towards the focus of attraction when at the nearest distance. And the axis always remaining parallel to itself, this effect will not alternate to the other pole, but become steadily predominant at the one, up to some certain limit, being also, in the latitudes near the pole, less affected by the diurnal rotation, or by the centrifugal force generated in it, and, at the pole itself, not affected at all directly by the daily tides. At the opposite part of the orbit, when at the greatest distance from the sun, the proportion of the effect at the opposite pole, measured relatively in comparison with the mutual attraction of the solid and fluid parts of the globe itself upon one another, will be greatly diminished, in consequence of the greater distance interposed by the eccentricity, the force of the gravitation being diminished in proportion to the increased

square of the distance. Again, near the quadratures of the orbit, the effect will be inclined to neither side, but remain undisturbed; and thus the steady predominance of effect in the accumulation of fluid will be at the pole which is turned to the sun at the time when nearest to that focus of attraction.

This serves to explain why the hemisphere of the earth towards the south pole, being turned towards the sun at that portion of the orbit where the earth approaches nearest to it, has so much greater a proportion of water than the northern¹. And, on this principle, it appears that the time never has been, and never will be, which some geologists speculate about in their glacial theories, when the pole having the preponderance of *land* would be turned towards the sun at the perihelion; but the extremely slow change in the line of the apsides, which they suppose would bring this about, must be always accompanied and balanced by a corresponding change in the geographical condition of the globe, as regards the distribution of land and water.

The result of this, if my position is correct, is to explode all glacial theories based upon the varying eccentricity of the earth's orbit, since apart from a preponderance of land at the pole turned towards the sun at the perihelion, Sir John Herschel and Sir Charles Lyell, with Mr. Beech, have shown confessedly that no such effect would follow, but the contrary².

¹ The Astronomer Royal for Scotland, Professor C. Piazzzi Smyth, in his interesting and valuable pamphlet "On an Equal-surface Projection for Maps of the World" (Edinburgh, 1870), has calculated that the parallel of latitude which equally divides the land of the globe, north and south, is that of latitude 25° N.

² Sir Charles Lyell's "Principles of Geology," 10th ed. vol. i. pp. 270. 273. 283.

PROPOSITION X.

THE fossil remains of the rhinoceros and mammoth in Siberia, with other kindred facts, demonstrate a great and also a very sudden change of climate.

It is desirable to take the statement of these facts from authorities which will be respected by those most disposed to question this proposition. "It is not on the Obi nor on the Yenesei," says Sir Charles Lyell, "but on the Lena farther to the east, where, in the same parallels of latitude, the cold is far more intense, that fossil remains were first found in the most wonderful state of preservation. In 1772 Pallas obtained from Wiljuiskoi, in lat. 64°, from the banks of the Wiljui, a tributary of the Lena, the carcase of a rhinoceros (*R. tichorhinus*), taken from the sand in which it must have remained congealed for ages, the soil of that region being always frozen to within a slight depth of the surface. This carcase, which was compared to a natural mummy, emitted an odour like putrid flesh, part of the skin being still covered with short crisp wool, and with black and grey hairs. In allusion to the quantity of hair on the foot and head conveyed to St. Petersburg, Pallas asked whether this animal might not have inhabited a cold region of Middle Asia, its clothing being so much warmer than that of the African rhinoceros. Professor Brandt, of St. Petersburg, in

a letter to Baron Alexander Von Humboldt, dated 1846, adds the following particulars respecting this wonderful fossil relic :—‘ I have been so fortunate as to extract from cavities in the molar teeth of the Wiljui rhinoceros a small quantity of its half-chewed food, among which fragments of pine-leaves, one-half of the seed of a polygonaceous plant, and very minute portions of wood with porous cells (or small fragments of coniferous wood), were still recognizable. It was also remarkable, on a close investigation of the head, that the blood vessels discovered in the interior of the mass appeared filled, even to the capillary vessels, with a brown mass (coagulated blood), which in many places still showed the red colour of blood.’

“ Thirty years after the discovery of the rhinoceros by Pallas, the entire carcase of a mammoth was obtained, in 1803, by Mr. Adams, much farther to the north. It fell from a mass of ice, in which it had been incased, on the banks of the Lena, in lat. 70°; and so perfectly had the soft parts of the carcase been preserved, that the flesh, as it lay, was devoured by wolves and bears.” [“According to Mr. Adams, who visited the spot, and collected the remains two years after the occurrence, and to whom we are indebted for the account of the discovery, the escarpment of ice in which the elephant had been preserved extended two miles, and rose perpendicularly about 200 or 250 feet. On this ice, which is described as pure and clear, there was a layer of friable earth and moss about fourteen inches thick¹.”] “This skeleton is still in the Museum of St. Petersburg, the head, its integument, and many of the ligaments entire. The skin of the animal was covered first with black

¹ De la Beche, “Geological Manual,” p. 170.

bristles, thicker than horse-hair, from twelve to sixteen inches in length; secondly, with hair of a reddish brown colour, about four inches long; and thirdly, with wool of the same colour as the hair, about an inch in length. Of the fur upwards of thirty pounds weight was gathered from the wet sand bank. The individual was nine feet high, and sixteen feet long, without reckoning the large curved tusks; a size rarely surpassed by the largest living male elephants.

“It is evident, then, that the mammoth, instead of being naked, like the living Indian and African elephants, was enveloped in a thick shaggy covering of fur, probably as impenetrable to rain and cold as that of the musk-ox. The species may, as Cuvier observed, have been fitted by nature to withstand the vicissitudes of a northern climate; and it is certain that from the moment when the carcasses both of the rhinoceros and elephant (i.e. mammoth) above described were buried in Siberia, in latitudes 64° and 70° N., the soil must have remained frozen, and the atmosphere as cold as at this day.

“The discoveries made in 1843 by Mr. Middendorf, a distinguished Russian naturalist, and which he communicated to me in September, 1846,” continues Sir Charles Lyell, “afford more precise information as to the climate of the Siberian lowland at the period when the extinct animals were entombed. One elephant was found on the Tas, between the Obi and Yenesei, near the Arctic circle, about lat. $66^{\circ} 30' \text{ N.}$, with some parts of the flesh in so perfect a state that the bulb of the eye is now preserved in the Museum at Moscow. Another carcass, together with a young individual of the same species, was met with in the same year, 1843, in lat. $75^{\circ} 15' \text{ N.}$, near the river

Taimyr, with the flesh decayed. It was embedded in strata of clay and sand, with erratic blocks, at about fifteen feet above the level of the sea. In the same deposit Mr. Middendorf observed the trunk of a larch-tree (*Pinus larix*), the same wood as that now carried down in abundance by the Taimyr to the Arctic Sea. There were also associated marine fossil shells of *living northern* species, and which are moreover characteristic of the drift, or glacial deposits of Scotland and other parts of Europe. Among these *Nucula pygmæa*, *Tellina calcarea*, *Mya truncata*, and *Saxivaga rugosa* were conspicuous.

“So fresh is the ivory throughout Northern Russia, that, according to Tilesius, thousands of fossil tusks have been collected and used in turning; yet others are still procured in great plenty. He declares his belief that the bones still left in Northern Russia must greatly exceed in number all the elephants now living on the globe.

“Remains of the mammoth have been collected from the cliffs of frozen mud and ice on the east side of Behring’s Straits, in Eschscholtz Bay, in Russian America, lat. 66° N. As the cliffs waste away by the thawing of the ice, tusks and bones fall out, and a strong odour of animal matter is exhaled from the mud.

“Intelligence has been received at St. Petersburg (from an exploring expedition now in progress, 1866) that in the flat country near the mouths of the Yenesci, between lat. 70° and 75° N., many skeletons of mammoths retaining the skin and hair have been found. The heads of most of them are said to have been turned towards the south. The preservation of these, and other individuals before mentioned, in ice or frozen mud, is a fact which has a most im-

portant bearing on all speculations concerning the climate of the arctic regions, both at the time when these animals existed, and throughout the whole period which has since elapsed. There may have been oscillations of temperature, accompanying changes in the geography of the globe, or partly due to extinct phases of the precession of the equinoxes, or to various states of the ellipticity of the earth's orbit since the era in question, but *one thing is clear, that the ice or congealed mud in which the bodies of such quadrupeds were enveloped has never once been melted since the day they perished, so as to allow the free percolation of water through the matrix, for had this been the case, the soft parts of the animals could not have remained undecomposed.*"

Nothing can be more indisputable than this conclusion, and it is of much importance that it should be kept steadily in view.

"Rome," continues Sir Charles, "is the most southern limit to which the fossil bones of the mammoth have as yet been traced in Europe. Some were detected in 1858 in Monte Sacro, one of the seven hills of Rome, where they were recognized by M. Lartet among the mammalian remains obtained by Professor Ponzi from the gravel beds of that locality. Other specimens, as I learn from M. de Verneuil, have since been found in ancient alluvium on the banks of the Tiber, at Ponte Molle, associated with flint implements of contemporaneous date. We are not obliged, says Dr. Falconer, to suppose that this ancient elephant, which in Europe extended its range from the Tiber to the Lena, and in North America from Eschscholtz Bay to the Gulf of Mexico, was enveloped in every latitude with a thick covering of fur. 'The fine silky fleece with which the domestic

goat is clothed on the plains of Tibet, where the winter, at a height of 16,000 feet above the sea, is most severe, disappears entirely from the same animal in the valley of Cashmere.' ” (“ Principles,” vol. i. 183-6.)

With full allowance for the last conjecture (though it is but conjecture), and admitting considerable power of adaptation to difference of food as well as temperature, the general character and habits of the animal must be assumed to have been pretty much the same. The reindeer may, in former ages, have ranged much farther south than now, and may have thriven in milder climates, as it is known to have done, probably until driven far to the north by hunters. But the mammoth could not graze upon icebergs, nor support its huge frame on a few polar lichens². The natural inference from the facts is, that

* It is amusing to observe the different estimates of scientific men according to their inclination. An elephant in Noah's ark must necessarily eat a great deal; but in the Arctic regions they say, “ It is clear that the quantity of food required by the larger herbivora is much less than we have usually imagined ” (Lyell's “ Principles of Geology,” vol. i. p. 191). For my own part, the parallel seems to me very close in this respect between them and the calumny against the knights of heroic romance, of whom it is said,—

“ And though knights-errant, as some think,
Of old did neither eat nor drink,
Because when thorough deserts vast
And regions desolate they past,
Where belly-timber, above ground
Or under, was not to be found,
Unless they grazed, there's not a word
Of their provisions on record,—
Which made some confidently write
They had no stomachs but to fight:
'Tis false.”

It might be unjust to Sir Charles Lyell not to insert his candid

the temperate zone had a greater range than now ; that Siberia was warmer, the Gulf of Mexico cooler ; and that the change was sudden. The animals were, many of them, overtaken in their ordinary haunts by a climate in which they could not possibly have lived ; they were frozen up in it, and, from that day to the day of their discovery, they continued so frozen up. Many of them were buried deep in the soil, many swept away in freezing water, drifted among polar sea shells and fish, embedded among marine remains, and piled with erratic blocks. In other cases their bones and tusks were scattered about, drifted into caves, mixed up with deep clay and boulders, by some catastrophe which effectually stirred the confused mass together, like the plums in a vast pudding, to furnish forth for the geologists the future feast of reason. Tusks of the fossil elephant have been found thirty or forty feet deep in the earth in the neighbourhood of Kilmarnock, in what used to be called the diluvial deposit, the remnant of what is now thought an exploded geological superstition. Similar remains have been found in various parts of England, as at Fisherton, near Salisbury, coupled

admission : " It would doubtless be impossible for herds of mammoths to subsist at present throughout the year even in the southern parts of Liberia, covered as it is with snow during the winter ; but there is no difficulty in *supposing* a vegetation capable of nourishing these great quadrupeds to have once flourished between the latitudes 40° and 65° N." There may be no difficulty in supposing it, nor that they went south in winter : though there might be some incongruity in supposing that their mishap occurred in the warm summer season. But the only difficulty is in supposing that it was, next moment in the life and experience of the same mammoth, a glacial period with ice 200 feet thick, and that that glacial period was the result of the extremely slow and imperceptible variation in the ellipticity of the earth's orbit.

with the remains of the Siberian rhinoceros, the tiger, hyæna, horse, and other extinct and living species. Bones also of the hippopotamus, of a species closely allied to that now inhabiting the Nile, are often accompanied, in the valley of the Thames, and elsewhere, by a species of bivalve shell (*Cyrena fluminalis*), now living in the Nile, and ranging through a great part of Asia as far as Tibet, but quite extinct in the rivers of Europe. Some zoologists conjecture that the fossil species of hippopotamus was fitted for a cold climate, but Sir Charles Lyell thinks it more probable that when the temperature of the river water was congenial to the cyrena above mentioned, it was also suited to the hippopotamus³.

Men of science may indeed strain hard to make out that the mammoth was adapted for an arctic climate, a meet companion for the reindeer⁴. Its wool is supposed favourable to this, but not necessarily. It is granted that the coat of the mammoth makes it probable that it was adapted not exclusively for a warm climate, but for the wide range of the temperate regions of the earth.

But the question is, how come the remains of these animals to be found so abundantly among the Siberian ice and frozen soil? This does not admit of being solved by the mere supposition of a flood occurring in a Siberian river while the lower portion of the river was blocked with ice towards its northern extremity; nor is it solved by the story told by a missionary, Huc, of having seen a troop of fifty oxen in a frozen river, which they had attempted to cross, whether by swimming or upon ice and snow not

³ Sir C. Lyell, "Principles of Geology," vol. i. p. 194, 10th ed.

⁴ Quoted by Lyell from a distinguished authority.

strong enough to bear them, and in which they became embedded and frozen up. To make the case parallel, that ice should be shown never to have melted in several thousand years, and not an ox of the species should have survived to tell the tale of its comrades. The determined overlooking of the evidence of the suddenness is an extraordinary example of what would be considered stolid blindness and bigotted prejudice, if it were not in philosophers. "Von Baer, of St. Petersburg, states that the ground is now frozen permanently, to the depth of 400 feet, at the town of Yukutz, on the western bank of the Lena, in latitude 62°. Middendorf told Sir Charles Lyell, in 1846, that in his town in Siberia, three years before, he had bored to the depth of 70 feet, and, after passing through much frozen soil, had come upon a solid mass of pure transparent ice, the thickness of which, after passing through two or three yards, they were unable to ascertain. The Rev. Mr. Brown, in his book on 'Icy Caves,' states that Von Kotzebue and his party, having encamped on a large plain in Siberia, discovered a fissure, which revealed the fact that the moss and grass on the surface was a thin coating on a layer of ice, 100 feet thick; that this was not mere frozen ground, but aboriginal ice, which formed the walls of the fissure. They found the bones and teeth of the mammoth embedded." I take these facts from the statement of Mr. J. Scott Moore. They illustrate the impossibility of those large animals having found subsistence in such a climate, and on a soil so constituted as that of Siberia now is, where their remains are so abundant. What then is indicated by their existence there? A still more glacial climate at some earlier date? Why is it not cold enough

now ? What need of going back 250 or 280 thousand imaginary years to find a supposed colder epoch and draw a picture of an enormously larger ice-cap ? The difficulty is, how these creatures lived there, and found food and warmth to live, until they were frozen up, since which sudden time the ice has never thawed. Whence this great change of climate ?

Some theorists, among whom we must give a prominent place to Mr. John Carrick Moore, whose reasonings Sir Charles Lyell mentions with approval, himself largely agreeing with them or going beyond, and also Mr. J. Scott Moore, in his work on "Pre-glacial Man," have attributed the change of climate to the effect of the increased eccentricity of the earth's orbit at certain epochs. But the variation of the eccentricity is one of the slowest changes in nature, requiring not centuries but thousands of years to be appreciable in its effects,—if even then ; for it has been remarked in Proposition IX. that there are conditions which would preclude it from producing the supposed glacial effects even at the maximum eccentricity. But Mr. J. Scott Moore thinks that for the supposed glacial period which froze up the mammoth we must go back 210,000 years ; and yet the change of climate was so slow that the ice did not break up and disclose the mammoth until our own day. Mr. Moore thinks it might keep fresh and good all that time preserved in ice. Sir Charles Lyell thinks the glacial time may more probably have been 750,000, or from that to 850,000, nay, even a million years ago⁵ ; but, not to enter into the consideration of that difference, so trifling a matter to these geologists, it does not appear to have been

⁵ Vol. i. pp. 295. 297 compared with table, p. 293.

considered by them that the difficulty was not how the mammoth kept fresh in the ice in which it was frozen up for all that length of time (though Sir Charles has shown us that, even without thawing, there is a certain evaporation of ice and snow), and though a million, or 750,000, or even 210,000 years must be admitted to be a long time; but the main difficulty is how the creature managed to stand grazing so long a time on the banks of the Lena while the cold was so gradually coming on; for whatever it fed upon, and whatever its covering of hair and wool, it certainly did not graze among polar icebergs, nor feed upon frozen cockles. But the change of the eccentricity is so slow that it is not likely that any conjectural increase of cold from that source could have taken less than perhaps ten thousand years to freeze it up, after the climate had ceased to be fit for the supply of its ordinary food. Now this may be a short time for the geologists, but it was a long time for the mammoth; and it is to be remembered that it is not a solitary case. They may say that that animal had been probably swept down by a flood of the Lena from a somewhat warmer latitude, but that supposition would not suit all the cases. There is evidence of a great change of temperature, and with all deference to the obliviousness of those who are so familiar with millions of years, flesh and blood in the mammoth could not subsist upon that nonsense. There is absolute and demonstrable certainty that the change of temperature was sudden; and though the eccentricity is now small, the change of climate, which was introduced suddenly, has continued permanent.

In allusion to some similar astronomical theories of very slow causes supposed to have operated, the

illustrious Cuvier remarks as follows: "As these motions are exceedingly slow they are altogether inadequate to account for catastrophes which must necessarily have been sudden. The same reasoning applies to all other slow motions which have been conceived as causes of the revolutions on the surface of our earth, chosen doubtless in the hope that their existence could not be denied, as it might always be asserted that their extreme slowness rendered them imperceptible. But it is of no importance whether these assumed slow motions be true or false, for they explain nothing, since no cause acting slowly could possibly have produced sudden effects⁶." Modern geology, however, seems to have fallen into the hands of persons to whom Cuvier is almost as much out of date as the Bible. But though thus with them, like faded dreams of their childhood, not so let it be with all.

Prior to that sudden change the climate of England was warmer, as well as that of Siberia; the range of the temperate zone extended farther north; there was in many parts a different relative level of land and water; and the change itself is thought to have occurred within the period of man upon this earth. Having noticed the eccentric glacial theory, with its easy coinage of many hundreds of thousands and millions of years, which, as Cuvier observed⁷, some naturalists lay prodigious stress upon, and can call them into action by a stroke of the pen, we may only remark that difficulties are not solved by being removed hypothetically a hundred thousand or a million years off, nor softened by having one or two

⁶ Cuvier, "Theory of the Earth," Prelim. Obser. § 18, p. 37.

⁷ "Theory of the Earth," with Professor Jameson's notes, Edin. 1815, p. 123.

hundred thousand hypothetical years interposed as a break between successive steps of the difficulty, and it were worthier of men to seek such causes as will fit and harmonize the facts.

The overlooking of the evidence of the suddenness of the catastrophe is the more singular and the less excusable, because the illustrious Cuvier had most expressly called attention to that point. "In the northern regions," says he^s, "it has left the carcasses of some large quadrupeds which the ice had arrested, and which are preserved even to the present day with their skin, their hair, and even their flesh. If they had not been frozen as soon as killed they must quickly have been decomposed by putrefaction. But this eternal frost could not have taken possession of the regions which these animals inhabited, except by the same cause which destroyed them; *this cause, therefore, must have been as sudden as its effect.*" Sound science is here, as always, consistent with common sense.

^s "Theory of the Earth," Preliminary Observations.

PROPOSITION XI.

THE strata containing the vestiges of the former life and sudden extinction of the mammoth, and other animals, belong to the period following the Chalk Formation. And all the strata subsequent to the chalk contain the remains of some of the common existing forms of life.

The fossiliferous strata in their order of succession and superposition, proceeding from the surface downwards, are thus arranged by geologists¹:—

1. Recent.	}	POST-TERTIARY.
2. Post-pliocene.		
3. Newer Pliocene.		
4. Older Pliocene.	}	TERTIARY.
5. Upper Miocene.		
6. Lower Miocene.		
7. Upper Eocene.		
8. Middle Eocene.		
9. Lower Eocene.		
10. Maestricht Beds.	}	(UPPER SECONDARY) CRETACEOUS.
11. White Chalk.		
12. Upper Greensand.		
13. Gault.		
14. Lower Greensand.		
15. Wealden.		

¹ Lyell, "Principles of Geology," vol. i. p. 139.

A variety of other strata follow, all of them fossiliferous, down to No. 39. The fossils of the supercretaceous strata are classed and designated as *Cainozoic*, or of the new forms of life; those of the chalk, and lower than the chalk, are classed as *Mesozoic*, that is of the intermediate forms of life; and, lower still, those of the carboniferous, Devonian, Silurian, Cambrian, and Laurentian Strata, are called *Palæozoic*, or of the older forms of life. In the supercretaceous strata the terms, *pliocene*, *miocene*, and *eocene* have been invented to express respectively the case—(1.) where the majority of the forms are of the common known forms of life; (2.) where the common known forms are fewer; and (3.) where there appears only, as it were, “the morning dawn” of those common forms, more sparingly distributed, and mixed with a preponderance of forms not now occurring.

Hugh Miller was of opinion that Geology allows of no break or gap between the Tertiary and Human periods, but that they formed one continuous duration, and he founded his opinion on the existence of some animals and shells common to the two periods. Some, as D’Orbigny, have questioned or denied this continuity up to the appearance of man; but we need not discuss it.

“It was long considered,” says De la Beche, “that the remains of elephants, rhinoceroses, and mastodons were confined to superficial gravels; but we now know that they are entombed deeper in the series of rocks, and were probably inhabitants of the globe before the *Palæotherium* and some other mammiferous genera became extinct².” They have been found

² De la Beche, “Geological Manual,” p. 192.

mixed with fluviatile and terrestrial reptiles, and with cetacea and other marine remains³. They have been obtained from the lignite of the Swiss molasse. According to Desnoyers, a mixture of bones of the *Lophiodon* and *Palæotherium* with those of the *Mastodon tapiroides* and middle-sized rhinoceros are found, accompanied by terrestrial and fluviatile shells, at Montabuzard. The facts stated are considered by De la Beche to show that the mastodons, rhinoceroses, and hippopotami existed as genera at the same time with the *Lophiodon* and *Palæotherium*, and that the former continued to inhabit certain parts of Europe when many molluscous animals existed similar or analogous to some of those contemporary with ourselves. Great mammalia are stated to be found in the blue marl of Italy, at Peruggia, Parma, and the Val di Metauro, as also in the sandy deposits of other places of the same country⁴. The gypsum beds of Paris have produced many sorts of the *Palæotherium* and *Anoplotherium*⁵. Professor Pusch, describing the rocks of Podolia and Southern Russia, states that near Krzeminec, in Vollynia, (where mountains rise above a plain covered with chalk, flints, and sand,) upper supercretaceous sandstone, occupying a thickness of 396 feet above the river Ikwa, and sixty feet beneath it, is composed of—

- (1.) Twenty feet of sand, cemented by a little carbonate of lime, containing many small shells and madrepores, the latter approaching *M. cervicornis*.
- (2.) Forty feet of calcareous sandstone, containing many shells of the genera *Cardium*, *Venericardia*, and *Arca*.
- (3.) Sixty feet of a compact quartzose and porous sandstone, the cavities filled with sand; contains

³ De la Beche, "Geological Manual," p. 194.

⁴ Ibid., p. 196.

⁵ Ibid., p. 226.

many *Venericardiæ*; lowest part most calcareous. (4.) Eighty feet of a marly limestone, containing many striated *Modiolæ*, *Pectens*, and other shells. (5.) At sixty feet beneath the surface, a quartzose and slightly calcareous white sandstone, containing numerous *Venericardiæ*, *Trochi*, and *Paludina*, or *Phasianellæ*. "According to M. Jarocki, while sinking a well in June, 1829, the tusk and molar tooth of an elephant were found in the last mentioned bed (No. 5), which are now preserved in the museum of Krzeminec. Many other bones were also observed, but they were too firmly fixed in the rock to be extracted" (Pusch, *Journal de Géologie*, t. 2). M. Pusch further remarks that this rock is the same, both mineralogically and zoologically, with the tertiary sandstone of Szydtow, and Chmielnik, in Poland; and that this fact is analogous to the occurrence of an elephant's molar tooth and tusk in the tertiary sandstone of Rzaka, Wieliezka, which contains *Pecten polonicus*, *Saricavæ*, and many other marine shells. It also corresponds with the occurrence of the remains of the great *Pachydermata* previously noticed as found mingled with marine exuviæ in other parts of Europe⁶.

M. Robert, describing the position in which numerous bones have been discovered at Cussac (Haute Loire), mentions that marls, without fossils, rest on the granitic rocks of the country. At Solilhac these marls are surmounted by clayey marls about two or three feet thick, containing plates of mica, grains of quartz, volcanic ashes, basaltic gravel, and impressions of gramineous plants; they also contain the entire skeletons of unknown Deer and Aurochs, with

⁶ De la Beche, "Geological Manual," p. 238, 239.

other bones. Above these are beds of volcanic sand two or three yards thick, with small basaltic and granitic pebbles, containing the remains of Ruminants and Pachydermata, the bones being more or less broken. On these rest alluvions of greater solidity composed of the same volcanic sand, large granitic and basaltic blocks (of which the angles are not rounded), geodes of hydrate of iron, and bones, which appear to have been exposed to the air before they were enveloped. All these substances are cemented by oxide of iron, and beds of ferruginous sand either alternate with, or repose on the alluvions. M. Robert extracted from these ferruginous beds at Cussac the remains of the *Elephas primigenius*; the *Rhinoceros leptorhinus*; the *Tapir Arvernensis*; the Horse, two species; Deer, seven species (to two of which he assigns the names of *Cervus Solilhacus*, and *C. Dama Polignacus*); the *Bos Urus* and *Bos Velaunus*, and the antelope⁷.

The progress of observation and discovery has added many details of fact, but they will be found confirmatory of our proposition. The mixture, also, of the remains of the fossil elephants and other extinct mammalia with marine exuviae, and with transported blocks and boulders, should not be overlooked.

⁷ De la Beche, "Geological Manual," p. 244.

PROPOSITION XII.

THE origin of the Chalk and Flint Formation has not been satisfactorily accounted for by geologists.

Sir Charles Lyell, blowing hot and cold in the same breath, has the following as the only references to the Chalk in the index of his "Principles":—

"Chalk: Warm climate indicated by the fossils of the chalk" (i. p. 213).

"Chalk: Floating ice in the sea of" (i. p. 216).

Nothing could be more refreshing in a tropical sea than a supply of that cooling luxury. Nevertheless, we do not meet with it floating there, except in the geological Utopia. The plants of the Upper Cretaceous formation of Europe point, as Sir Charles informs us, to the existence of a high temperature. Those of the Lower Cretaceous group, though differing, are such as favour the hypothesis of a warm climate. The huge saurians also, and other reptiles of the chalk, give the same tropical indications. But then there are other facts. "The occasional discovery of single and perfectly isolated stones, usually consisting of quartz and green schist, in (the white chalk of) the south-east of England has naturally excited much surprise." Assuming the chalk to have been formed at the bottom of an open sea, Sir Charles

asks, "In what manner could such stones have been carried far out into an open sea, so as to fall to the bottom without any intermixture of other foreign matter?" Abandoning his first hypothesis, that they had been carried out adhering to the roots of floating trees—chiefly on the ground of a block of syenite found, with granitic sand and pebbles of greenstone, in the white chalk at Purley, near Croydon, and which was too heavy for transportation by drift timber—he adopts the suggestion of Mr. Goodwin-Austen, that they must have been frozen into coast ice, floated out to sea, and deposited on the melting of the ice. Mr. Goodwin-Austen remarked that this would not necessarily imply a greater degree of cold than has occasionally been experienced on the east coast of England in our times. But then, by the hypothesis, it was not at all like the present temperature of England in some exceptionally severe season, but was a warm climate, not to say sub-tropical. I do not know whether the difficulty might not be disposed of by Sir Charles in the usual manner, by interposing a hundred thousand or a million years between the chalk and the syenite, and connecting the latter with the glacial epoch. A few hundred thousand years are nothing to a geologist, and may be cheaply obtained; and the same hypothesis which serves for an elephant may do equally well for a bit of syenite, which would also be better able to wait for the slow coming on of the supposed glacial period at the rate of the increasing eccentricity of the earth's orbit for the hundred thousand years or so required. Perhaps this supposition might put us in error fifty or a hundred thousand years—but that is nothing to a geologist. A geologist, with his enlarged views, may safely despise and smile at the narrow-

mindedness of those who can deal only with limited measures of time, and who prefer proof to hypothesis. The vast infinite of past time is before the geologist. When Mr. J. Scott Moore thinks he has proved that man existed before the glacial period, which he places some two hundred and fifty thousand years ago, and Sir John Lubbock that man was contemporary with the mammoth, which perished at that supposed remote glacial period, why might not some of them suggest that a boatful of these men had ballasted their boat with syenite, granitic sand, and greenstone pebbles, and, finding they lay too deep in the water, had thrown overboard a bit of syenite, with some other ballast of sand and pebbles? Why should we be limited to ice or tree-roots, if man was upon the earth so long ago? The pity is only that geology should have been reduced to a science of conjecture, a grand factory of hypothetical centeno-millenaries, and of ivory two hundred and fifty thousand years old, as fresh and fit for the turning-wheel as if it were but of yesterday.

But the whole difficulty of that bit of syenite is very small, compared with the difficulty of these proposed solutions, which are indeed only *ignotum per ignotius*. We shall pass on, therefore, to the question how the chalk came there at all, and how the flints came with it, or got into it.

Chalk is supposed to have been formed at the bottom of the sea, and to consist of, or be the production of, innumerable millions of infusoria foraminifera, and microscopic shell-fish. "The homogeneous character of the white chalk, or upper portion of the great cretaceous formation, throughout a large part of Europe," observes Sir Charles Lyell¹, "is

¹ "Principles of Geology," vol. i. p. 216.

now explained by the discovery that it is made up almost exclusively of the remains of the calcareous shells of Foraminifera, while the silicious portions have been derived chiefly from plants called Diatoms. It was ascertained when soundings were made for the Electric Telegraph, that calcareous mud of a similar character and origin is now forming over vast areas in the depths of the Atlantic." Perhaps it would be more correct to say, simply, that it is now there, than that it is "now forming." Do the animals whose remains are found in the chalk live at a depth of two miles? And with regard to Infusoria, it is not surprising to find traces of them any where, since they have been found abundant in lavas thrown up from untold depths², and exist plentifully in the air itself in some instances, in which the germs of vegetable and animal life float continually, and the developed forms of insect and microscopic life abound. But where did these minute creatures obtain so much carbonate of lime? You must give them straw to make bricks. Sir Charles Lyell is not unacquainted with the well-known fact (for he quotes it³), that if a common hen be altogether deprived of calcareous nutriment she will lay her eggs without shells, having no material for them. And he also admits the same necessity of a supply of materials for the operations of crustaceous animals, corals, and marine plants⁴. They could not create the substance; and if they shaped and modified it, the question is left, how came there to be such a prodigious quantity of carbonate of lime at one particular epoch; and if held in solution in the waters of

² "Principles of Geology," vol. i. pp. 644, 645.

³ Ibid., vol. ii. p. 610.

⁴ Ibid., vol. i. p. 450.

the ocean, why limited to certain definitely bounded tracts? That chalk falling into the sea should speedily become animated with multitudinous forms of life, and turned to account for the uses of innumerable shell-fish, does not give any account of the origin of the chalk. Life every where abounds, and the rapid development of those forms of life is such as almost passes imagination. But if a man of some primitive race, unacquainted with the nature and manufacture of cheese, should, on beholding a cheese full of mites, make the surprising *discovery* that the cheese was the production of the mites, his conclusion would be of the same sort with theirs who ascribe the whole mass of the chalk to the numerous shell-fish and infusoria which have made use of it. If wheat flour be macerated, infusoria may be developed; but are the infusoria to be accepted as accounting for the origin of the flour?

The Flint Formation goes hand-in-hand with the Chalk, and each must be taken into account conjointly with the other in a satisfactory investigation of either. Flint was at one time supposed to have been formed by an electrical segregation and mutual attraction of silicious particles making their way into, and filling up little cavities in, the chalk, which had been formed by the disengaging of carbonic acid gas, like the holes formed in bread in the process of rising. That hypothesis seems to be abandoned. Ehrenberg thought that the flints were formed of the silicious wings or cases of microscopic infusoria. Mr. J. S. Bowerbank, in a paper on the silicious bodies of the chalk, greensands, and oolites⁵, gives the result of some microscopic examinations of thin slices, and

⁵ "Geological Trans." 1840, 2nd series, vol. vi. p. 181.

argues in support of their organic origin; but the facts stated by him will not bear out any such general conclusion, being drawn from casual, not uniform appearances; as from traces of reticulated structure, like sponge, only in parts; from the occurrence of spicula and foraminifera in parts; from occasional silicified echinidæ, and from some flints enclosing nuclei of sponges or corals.

But the more favoured theory now is, that the flints have been deposited at the bottom of the ocean from thermal springs impregnated with silica. These springs are supposed to have deposited the silica in the manner of the Geysers of Iceland, forming incrustations around various substances as nuclei, and also infiltrating the substances themselves, so as generally to transmute the whole to silica. It is obvious, however, that this theory is based upon those instances in which some foreign substance capable of being supposed a nucleus is discoverable in the flint; instances which are the exceptions, not the rule. It is obvious also that it does not account for the occurrence of flints in separate and successive layers in the chalk. Further, it is based upon an unfounded conjecture of the existence of innumerable thermal springs at the bottom of the ocean of the chalk over a vast area, or else of a thermal silicified ocean. Sir Charles Lyell observes⁶, that "in order that water should hold a large quantity of silica in solution it is necessary that it should be raised to a very high temperature:" and what he means by "a very high temperature" may be understood by the instances which he gives of the hot springs of the Valle das Fernas, in the island of St. Michael,

⁶ "Principles," vol. i. p. 408.

rising through volcanic rocks, which precipitate vast quantities of silicious sinter. "Around the circular basin of the largest spring," says he, "which is between twenty and thirty feet in diameter, alternate layers are seen of a coarser variety of sinter mixed with clay, including grass, ferns, and reeds in different states of petrification. In some instances alumina, which is likewise deposited from the hot waters, is the mineralizing material. Branches of the same ferns which now flourish in the island are found completely petrified, preserving the same appearance as when vegetating, except that they acquire an ash-grey colour. Fragments of wood, and an entire stratum composed of reeds now common in the island, have become completely mineralized. The most abundant variety of silicious sinter occurs in layers from a quarter to half an inch in thickness, accumulating on each other often to the height of a foot and upwards, and constituting parallel, and for the most part horizontal, strata many yards in extent. This sinter has often a beautiful semi-opalescent lustre. A recent breccia is also in the act of forming, composed of obsidian, pumice, and scorix, cemented by silicious sinter." It may be observed that there is nothing here resembling the deposit of successive layers of flint nodules. The Geysers of Iceland are also instanced as illustrating the deposition of silex now in progress. "The circular reservoirs into which the Geysers fall are lined in the interior with a variety of opal, and round the edges with sinter." "The plants incrustated with the latter substance have much the same appearance as those incrustated with calcareous tufa in our own country." "By analysis of the water Mr. Faraday has ascertained that the solution of the silex is promoted by

the presence of the alkali, soda. He suggests that the deposition of silica in an insoluble state takes place partly because the water when cooled by exposure to the air is unable to retain as much silica as when it issues from the earth at a temperature of 180° or 190° Fahr.⁷; and partly because the evaporation of the water decomposes the compound of silica and soda which previously existed. This last change is probably hastened by the carbonic acid of the atmosphere uniting with the soda. The alkali, when disunited from the silica, would readily be dissolved in and removed by running water.” “Mineral waters, even when charged with a small proportion of silica, as those of Ischia, may supply certain species of corals, sponges, and infusoria with matter for their silicious secretions; but there is little doubt that rivers obtain silex in solution from another and far more general source, namely, the decomposition of felspar⁸.”

I have extracted these particulars that I may do no injustice to the prevailing theory of the formation of flint nodules, and yet exhibit the fact that the productions of silicious springs are very dissimilar in form and nature.

An ocean of good hot water as far north as Norway may, no doubt, be a grand conception, and very suitable to the purpose of those who are accustomed to assail the historical facts of Scripture with quasi-

⁷ The following fact is still further illustrative of temperature:— “At the mouth of the central tube of the little Geyser the water has a temperature of about 212° Fahr.; but at a certain depth it is much hotter, a thermometer, suspended by Professor Bunsen with a string down the tube, having risen to 266° Fahr., or 48 degrees above the boiling point.”—Lyell, “Principles,” vol. ii. p. 220.

⁸ Lyell, “Principles of Geology,” vol. i. p. 408—410.

scientific conjecture. But like the glacial conjecture, it has its difficulties; and it may naturally be said, if the slow, myriennial approach of the *cold*, would not suit the possibility of the elephant's grazing till it was frozen up, so neither could a thermal ocean, of the required temperature for holding such quantities of silica in solution for rapid local deposit, and far too hot we should fancy even for the huge moso-saurus and its contemporary living races, though of tropical kinds, be likely to carry *ice* upon its surface.

It has been found in some cases that flint encloses a pebble of chalcedony in its centre, as in other cases it may enclose some other substance; and though it does not necessarily follow that the chalcedony was in every case formed at the same time, or by the same process with the flint, or is in a correct sense part of the flint itself, yet the vegetable remains contained in some specimens of chalcedony, formed and found elsewhere, have been quoted to illustrate the substances enclosed in flint, which they do not generally resemble. It may be desirable, therefore, to advert to this subject.

Chalcedonies and agate nodules are found in some volcanic rocks, as in Basaltic geodes and in Trap. It has been questioned, therefore, whether they are not of igneous origin. In an interesting paper in the Geological Transactions (1st Series, ii. 511) "On Vegetable Remains, preserved in chalcedony," by Dr. MacCulloch, formerly Vice-President of the Geological Society, the author, though adhering to the theory of aqueous formation, and therefore interpreting the plants (though he could not determine the species) as being *confervæ* of some sort, or mosses, or ferns, or lichens, or some of the

plants contained in the troublesome class of cryptogamia, appears in some respects not by any means satisfied with the aqueous theory. He distinguishes with care many cases in which the arborescent forms are merely the whimsical and fibrous disposition occasionally assumed by chlorite, just as by water in freezing, and by various metallic substances on crystallizing, and remarks the great liability to be deceived; and he illustrates both these, and the real cases of vegetable remains which he had tested and found to be such, with coloured drawings. After remarking that some of the chalcedonies are formed by the stalagmitic process, Dr. MacCulloch adds, at page 521, "In many others no mechanical structure or disposition can be discovered in the stone, but the whole appears one semi-transparent mass of chalcedony." *These* are the specimens in which the *confervæ* [or whatever the plants are] preserve that freedom of arrangement, and perfection both of colour and structure, which seem to imply that they had been so *suddenly* involved in a mass of silicious matter, as to have been preserved from all future changes. The most beautiful and decided vegetable specimen which his drawings represent he considers to be a fibrous *root*, not a floating water-plant. He mentions that Daubenton had determined two of the plants to be *Lichen digitatus* and *Lichen rangiferinus*. These are not strictly water-plants, much less do they agree with formation at the bottom of the ocean. The latter of the two is the main subsistence of the reindeer. He produces an instance of a *chrysalis* imbedded in a striped onyx agate, mounted in a ring which was in the possession of the Earl of Powys. That certainly does not indicate a formation under water. And in the conclusion of his paper he ob-

serves, "It is plain, on reviewing some of the cases above described, that a process different from the tedious one of infiltration must have produced the appearance in question. Neither the free disposition, nor the forms of the delicate vegetable structure, could have been preserved during so slow a process; nor, could their colour have remained unaltered. The loss of colour must have followed the death of the plant, and the total loss of its figure would have resulted from the gradual changes which it must needs have undergone during the continuance of a process so tedious. The remains are in fact, if I may use such an expression, embalmed alive."

I may place in conjunction with these remarks of Dr. MacCulloch, a similar observation of Dr. Carpenter, in regard to his very beautiful microscopic discoveries on the structure and affinities of the Eozoön Canadense. The shelly chambers of this minute organism remain, but the fleshy substance, or sarcode of the animal has been replaced by serpentine, or some other silicious mineral, and by removing the calcareous chambers with an acid Dr. Carpenter obtained the model of the microscopic animal itself in serpentine, on which, of course, the acid took no effect. Now of this he remarks, "Even the hue of the original sarcode seems traceable in the canal-system, so exactly does its aspect, as shown in transparent sections, correspond with that of similar canals in recent specimens of *Polystomella calcarina*, &c., in which the sarcode body has been dried. This last circumstance appears to me to afford a remarkable confirmation of the opinion formed by Mr. Sterry Hunt upon mineralogical grounds—that the silicious infiltration of the cavities of the eozoön was the result of changes occurring *before the decomposition of*

the animal. And the extraordinary completeness of this infiltration may be the result . . . of the superiority of the process of *substitution*, in which the animal matter is replaced (particle by particle) by some mineral substance, over that of mere *penetration*.* I quote these things, be it observed, not as belonging to the period of the flint, but as illustrating facts and processes which may tend to the correction of erroneous inferences, in some cases, as to the manner of formation.

Since, then, it could not be done by the slow process of aqueous infiltration, is it not equally worthy of a thought whether, under no conceivable circumstances, it could take place from igneous fusion, if the molten mass suddenly enclosed the vegetable or animal substance, and, while consuming its substance, and substituting its own, retained the traces of its form, and appropriated there its colour? Air, we know, is generally necessary to combustion; but air might be wholly excluded, or its place supplied by carbonic acid gas. Might not some such circumstances, or others different still, prevent the combustion of animal fibres, until the instantaneous surrounding of them by the liquid silica had left, not the fibres, or the vegetable substance, but the accurate and delicate tracery of its form in the substituted silica itself? If this idea be rejected, let some other be substituted more tenable than that which Dr.

* I quote from a paper kindly presented to me by Dr. Carpenter, which is a reprint from the proceedings of the Royal Society of December 15, 1864, of a letter by him to the President, "On the Structure and Affinities of *Eozoön Canadense*." It will be observed that though Dr. Carpenter here uses the word *infiltration*, he uses it not in its ordinary sense, but to express *substitution*, not *penetration*.

MacCulloch dismissed as *impossible*. We know that certain substances prevent combustion, and may protect even delicate fibres for a sufficient moment. Professor Pepper has shown us a man walk unharmed with naked feet on red hot bars of iron; and human art cannot surpass the resources of nature.

But there are other supposable conditions. Silica, which is known to be by natural processes soluble to a certain extent in water, may, in the powerful alchemy of nature, be drawn into the atmosphere with the water *so impregnated*, or otherwise itself sublimated into vapour; and in that state its rapid and delicate impenetration of minute vegetable fibres would be more conceivable than by deposit from water. Nay, in that state of vapour its impregnation of water itself might take place. Now Dr. MacCulloch, in one of his communications to the Geological Transactions, has mentioned an unexpected result, which he had once accidentally obtained, but had never been able in subsequent experiments to repeat, of the sublimation of silica. Some years previously, he tells us, being in pursuit of another object, a mixture of the oxides of tin and lead was exposed to the heat of an air furnace in an English crucible, to the top of which was luted another of the same sort. It was on the fire some hours, but no account was taken of the heat. By and by the insides of the crucibles were found to be covered with white brilliant filamentous crystals lying in all directions, which proved to be crystals of silica. He could never afterwards obtain the same result on repeated trials. The rarity of the occurrence, he remarks, serves to prove that there are properties of silica with which we are yet unacquainted. In a geological point of view he thinks it worthy of record,

as not only establishing the volatility of silica, but serving to prove that this substance may be crystallized from a state of vapour, as sulphur, some neutral salts, and some metals are known to be. Possibly, he adds, we may thus gain a step on which to rest in the investigation of the difficult subject of mineral veins, and the arrangement of the crystallized substances which occupy their cavities. The possibility of also explaining by this process the crystallization of the delicate filamentous zeolites which occupy the cavities of amygdaloids will readily occur to every mineralogist¹.

Although, then, it appears from the fossil marine remains of the chalk that in many, if not in all cases², it had once lain at the bottom of the sea, or been covered with marine water; and although flints have in many cases enveloped sponges and corals, and received the impression of marine shells on their exterior surface, so that they must in innumerable instances have come to the bottom of a shallow sea in a soft state, this is all these things prove; but no consistent or tenable account has been given by geologists of the origin of the formation. Why should the flints be generally found in nodules and rarely in the tabular form? Vast multitudes of the smaller flints are spherical or ellipsoid: others larger are tuberoso with rounded bulbs and round or angular indentations, as if several in a semifluid state had run together, and pressed on others already hardened, or on angular stones. Some are of stalactitic shapes,

¹ "Geological Transactions," 1st series, vol. ii. p. 275.

² The Rev. Thos. Milner, F.R.G.S., in his "Gallery of Nature," p. 641, says that indications of *birds* and *monkeys* are found in the cretaceous formation; and that popular work contains only what is generally accepted.

as if the fluid and slowly solidifying silica had run over the projecting corner of a bank or ledge, and had hung occasionally in pendants of an inch or two, or a foot, before it dropped. Some are extremely irregular, as if a splash of congealing fluid matter had made its way where opportunity was given, indented often by round hardened flints, here and there perforated as if the semi-gelatinous flint had fallen on a spike of wood, or flowed round a smooth stone ; some having ferruginous sand and stones rolled up in their interior ; and though externally coated with white chalk, and in some cases incorporating patches of white chalk within them, yet lying in a mass of ferruginous sand and stones. The interior surface, enclosing the sand and small stones, is rough in correspondence with the enclosed substances, and the exterior surface comparatively smooth, though here and there indented. I have noticed in various cases spicula of the flint, which proved to have been caused simply by the soft flint finding a passage between the little stones rolled up in it with the ferruginous sand, and demonstrating, therefore, that these little stones and sand had been there while the flint was in the semi-fluid state, as well as ever since it hardened. These cases are not unfrequent near Dunstable, where the flints do not lie in the white chalk, but chiefly in a reddish ferruginous gravelly soil abounding in large irregular masses of flint not rolled, together with innumerable small spherical or oval flints. Since Sir Charles Lyell takes pains to speculate about a bit of syenite in the white chalk, perhaps it may interest him still more to inquire how a flint having an incrustation of white chalk, and patches of white chalk incorporated, should hold red ferruginous sand and stones in its interior, and give proof by the

filling of the silica between their interstices that they had been there before it hardened. How came the white chalk, if it was formed in ferruginous earth and sand? How came the red ferruginous earth and stones in the inside of it, if it was formed by aqueous deposit in pure white chalk at the bottom of the sea?

The flints occurring in a bed of organic remains in the chalk cliffs near Dover, examined by William Phillips, Esq., F.L.S., are described by him as generally of remarkable forms. "Not uncommonly," he says, "they are of a nearly spherical shape; and when solid there is uniformly, as far as my observation goes, a small indented circle upon each. When not solid they always contain a nucleus having the appearance of a sponge of the same shape with the flint. These rarely exceed an inch in diameter; others are cylindrical, and enclose another flint of the same form; others (and they are numerous) are conical, having a flat base, around which is always indented an oval, within which there is sometimes the indented mark of a sponge. Some of these are solid; others are lined with tuberculated chalcedony of a bluish aspect; these are about two inches high; a thin lining of blue chalcedony is by no means uncommon within some of the flints of this bed. There are others very common in it, whose external marks, consisting either of deep indentations or small rugged projections, bespeak the probability of their formation being in some way or other connected with organic matter. But there are other flints which it is not easy to describe. They enclose a cylindrical flint resembling the stem or a branch of a vegetable, which, passing along the mass, is visible at each end, where sometimes it divides into numerous little

branches. Another of the same description crosses it, giving to the external flint a peculiar shape, and inducing the belief that it must have been formed around some organic substance, of which the form is preserved by the internal ramifications. The whole of these flints are numerous in this bed of organic remains, but," adds Mr. Phillips, "I did not discover any resembling them in form and character in any other part of the chalk'."

In the Geological Museum at Cambridge there is an exceedingly beautiful, various, and extensive collection of the organic remains of the Chalk and Flint Formation, more particularly in the "Forbes Young Collection" there, surpassing, in this department of geology, what is exhibited in the cases of the British Museum, or the Geological Museum of London; and I have examined it with extreme interest. At the same time my own attention has been given not exclusively to the mysterious organic remains contained in the chalk and flint, but to the indications shown in the form and fashion of the flint itself; and in nothing which I have seen have I found aught contrary to what I have advanced, or am now about to advance.

* "Geological Transactions," 1st series, vol. v. pp. 16—27.

PROPOSITION XIII.

SOME substances occurring on the earth are known to be of meteoric origin; and the fall of a flint meteorolite, which is most circumstantially stated to have occurred on the 6th of November, 1869, at Fawley, near Southampton, is in harmony with the observed phenomena of the Chalk and Flint Formation.

Though long regarded as absurd and impossible, it is now beyond all possibility of dispute that many stones and masses of iron have fallen from the sky. One mass of meteoric iron was found in Peru on the surface of the ground, weighing fifteen tons. Pallas found one in Siberia. One was seen by many to fall in Croatia. The iron is of a peculiar character, and contains a certain portion of nickel, which is also found in many of the meteoric stones. In many cases the fall of the stone has been actually witnessed, preceded generally by a loud explosion. The peculiar characters of the stones which had been witnessed to have fallen facilitated the identification of others of the same description; but the observation of these may also have tended to restrict too narrowly the admission of the fall to those cases in which the same characters were found. Baron Humboldt has called attention to what indeed appears to be a

veritable tradition of the fall of a meteoric nugget, or nuggets, of gold, in Scythia, recorded by Herodotus in the fourth book of his histories. Whether the gold was afterwards fashioned into those implements which it is described in terms of, a plough, a yoke, an axe, and a cup, or whether some fancied resemblance in the casual forms of the nuggets originated those terms, the fall of the gold is expressly recorded to have been witnessed, and also the very characteristic circumstance that it fell burning hot; so that the first of three brothers, who saw it fall, and who went to it meaning to take it, quickly let it go; and the second, in like manner, found it too hot to hold, and let it go; but when the third came to take it, it had cooled¹. No doubt, by those who have arbitrarily and summarily settled the point that nothing *could* be a meteorolite but what contained iron and nickel, this case would be disposed of after their fashion, by interrogating the nugget. Did it contain these substances? Answer, we may presume, on application of the magnet, No. Was it of the approved specific gravity for meteoric stones? Answer, on the application of that test, Too heavy. Had it a black, glossy surface? Answer, Certainly not. Summary decision of the judges, Then it is not of meteoric origin:—Case dismissed, with a slur on Baron Humboldt's credulity. This is the probable style of dealing with it. But then it is not so very long since all meteoric stones were generally scouted as incredible; and there appears no other mode of accounting for how gold nuggets ever came into the ground, since no one has suggested their having been formed by infusoria or shell fish, or deposited by

¹ Herod, iv. 5—7. Humboldt, "Cosmos," i. 102.

auriferous hot springs at the bottom of the sea. Latterly meteorolites consisting of *carbon* have been found ; and of these there are some specimens in the British Museum, presented by Sir John W. F. Herschel, in 1845, which fell at Bokkeveldt, Cape of Good Hope, on the 13th of October, 1838 ; and by Sir John W. F. Herschel and Thomas Maclear, Esq., Astronomer Royal at the Cape, 1839 ; by E. Charlesworth, Esq., 1839 ; and from Montauban, in the department of the Tarn and Garonne in France, on the 14th of May, 1864. These are exhibited along with 260 meteorolites of various other sorts.

Nothing could be more suggestive than the fall of such a substance as carbon. Can it be a charred product of the timber or other vegetable matter of a disrupted world, or planetary islet ? And has the diamond ever been formed in the upper laboratory of nature, a small ultimate crystal, from the intense heat followed by some electrical condensation, in the progress of the fall of a carbon meteorolite ? All that we know of that is that the diamond is pure crystalline carbon, and is found in superficial gravels ; and diamonds as well as carbon meteorolites have been found at the Cape. But as our acquaintance with meteorolites becomes increased we need not wonder if some extension of the classes and kinds of substances known to have had an extra-telluric origin should from time to time occur. We meet with many round pebbles, not shaped by rolling and trituration, but having an igneous crust surrounding them. Whence come they, and are they all of telluric origin ? The appearances are rather favourable to a meteoric source, because the round or oval forms, since they have certainly not been obtained by rolling, would most readily be accounted for by the molecular

attraction acting in free space, aided by rotatory movement in falling. The thought is not new, but has suggested an entire theory of the earth to a continental writer, M. M. de Marschall², mentioned by Cuvier, in which it may very probably have been carried to excess. Others before him have taken a similar view. Even Leibnitz³ thought that the material of the calcareous formations abounding on the earth had been condensed from vapours, and deposited from the atmosphere; and Descartes is mentioned by Cuvier as of a similar opinion. That such men had no foundation in nature for these thoughts, though they may have pushed them beyond what that foundation warranted, would be an assumption too rash to be made without very careful examination. But proof from actual observation of the stone's meteoric fall is difficult to obtain, especially where the substance falling is of a familiar nature; for it would in most instances be passed over, in seeking for something unfamiliar, and marked by rare characteristics, such as the seeker had settled in his own mind from previous limited specimens; and though he should pitch his foot against it, if it were only a flint it would either not suggest to him the question how it came there, or he might unconsciously incline to the answer supposed by Paley, that, for ought he knew or cared, it might have lain there for ever. That an instance of the fall of such a meteorolite should be recorded is less likely than that a hundred should have been overlooked. Never-

² "Researches respecting the Origin and Development of the Present State of the Earth." Geissen, 1802. (Cuvier, "Theory of the Earth," Preliminary Observations, § 19.) I have not met with a copy of this work of M. M. de Marschall.

³ *Protogæa. Act. Lips.* 1683. *Gott.* 1749 (Cuvier).

theless if an instance of the kind be recorded, it ought not to be persistently overlooked afterwards.

The following letter descriptive of the fall and finding of a flint meteorolite was published in the "Standard" of November 18, 1869:—

THE METEORS.

TO THE EDITOR.

SIR,—As I have not seen in your paper any account of the falling of two meteors in this village, I write to tell you that such is the fact. On Saturday, November 6, two meteors were seen to fall perpendicularly from the sky almost at the same time, within a few minutes of seven o'clock, and about a mile distant from each other. The first, seen by six or seven people at least (no doubt the same as seen by your correspondent at Highclere), fell near Blackfield Chapel, and about sixty yards to the left of a man walking along the road towards that building. This all describe as being the most magnificent and varying in colour, leaving a long train of sparks behind, which were visible nearly ten minutes; after which, for full half an hour, a luminous vapour or cloud was seen assuming various shapes, crescent, sinuous, &c. (as seen from different places), and being gently carried along to the westward by the wind. It burst when near to the ground, and a mass of flame shot out from it (the man says he should think two yards wide), but he heard no report. When he first saw it falling it was small, but before its bursting it seemed as large as his double fists, and of the colour of the moon. The light given out was very great. Another person says "it made it as light as day." I have since been searching in the field, but have not been successful in finding any fragments.

The second meteor was seen by, I know, four different people, who thought it fell in the rectory-grounds (it has since been found in the field beyond). They compare it to a ball of fire, first of a bluish colour, changing as it fell to a deep orange, but leaving no sparks behind: this also gave a brilliant light. It was picked up on the Wednesday following by Mr. Slightam, who says it had then a strong sulphurous smell. It was an object of much interest to the people here.

I am afraid of making my communication too long, so I will simply say its outward appearance is similar to those generally

known as meteoric stones, but the upper part (that is, as it lay on the ground) had a piece seemingly blown off, and this shows the interior to be a pure flint of a brownish tinge: lying on this is a fine powder in crystals, like silica; others more needle-shaped; then also metallic grains are visible—nickel, I suppose. There is also a projection, which has the appearance of a broken fossil shell of the mussel class; but this we wish to have examined, and to do that the stone must be broken or cut open more than it is at present; for, should it prove to be such, it would go a long way to establish the theory of meteoric stones being part of a dismembered world. In size it is less than a cricket ball, and weighs more than 1lb. avoirdupois. It must have been in a soft state when it reached the ground, for it had not penetrated more than half an inch, and that part is much flattened and smoother than the rest.—I am, sir, yours respectfully,

A. T. SMITH.

Fawley, near Southampton, Hants, Nov. 16, 1869.

To this letter, after waiting some time in expectation that it would be noticed by some one more habitually conversant with such studies, I ventured to recall attention, in a letter which appeared in the "Standard" of December 11th, 1869, and which I here subjoin.

A STONE CRYING OUT.

TO THE EDITOR.

SIR,—In your journal of the 18th of November last there was an account of the fall of a flint meteorolite at Fawley, near Southampton, Hants, in a letter from A. T. Smith, Esq., of that place, which appears to me so remarkable, that, as I have not seen it subsequently noticed by those more directly conversant with geological studies, I venture, if you will permit me, to recall attention to it.

The meteorolites which have been noted hitherto, and recognized as such, have not been of that description. But the origin of the flint formation, consisting of separate balls in stratified layers

among the chalk, has been an unexplained mystery. The existence of shells, fossil sponges, and corallines, among the chalk, and even of corals imbedded in flint itself, of which there are specimens in the Museum, indicate that, in such instances, it must have been once under the sea. But as two-thirds of the globe are covered with sea, a large portion of the meteorolites may be believed to have fallen there. If a flint meteorolite, while yet in a soft state (and that which fell at Fawley is thought to have been soft when it reached the ground), fell upon a projecting coral, it would be pierced by it, and so having imbedded it, would probably break it off, and carry it along with it to the bottom. There are appearances which may favour the opinion of the formation of the flint by intense heat melting and separating the silica from a material containing other substances; the exterior crust of the flint having apparently, like felspar, some substance fused in combination with the silica, and with which it had therefore been in contact. By the intense heat of the meteor in its fall other portions of the material, divested of the melted silica, might be pulverized and precipitated in chalk dust. The forms of flints are such as agree with their formation, not in cracks and fissures of the earth, like siliceous veins, or by filtration and deposit of silicated thermal waters, but in free space, where they could, and naturally would, assume the globular form, variously modified by occasionally running together, also affected by dropping in a soft or semi-liquid state into undulating water. The effect of rapid cooling by the water, or in some cases collision with rocks, would account for some cleavages and fractured edges, which modify the generally rounded form.

What continuous and severe meteoric showers, like hail and snow storms, this planet may have been visited with in its earlier physical history we may thus have some indications of. The recent flint-fall bears testimony for its species. Detached or sporadic falls might occur later than the general showers; and, having regard to the sharp-edged and pyramidal forms into which I find flint tends to split when it has been exposed to a strong red heat⁴, I incline to think that the broken flints, which have been made so much of as supposed evidences of man's existence in long pre-Adamite ages, and of his construction at that time of rude spears and axes, are no more than natural splinters of some large sporadic flint meteorolites.

⁴ I observe that Mr. Whitley also notices this tendency, in his pamphlet afterwards cited, p. 18.

The drawings which Sir Charles Lyell has given of some of them from the valley of the Somme in his "Geological Evidences of the Antiquity of Man," pages 114, 115, impress me strongly with this idea. Sir Charles confesses that their sharp edges have certainly not been obtained in the manner in which any known human savages ever sharpened their stone implements. The evidence supposed to be afforded by them to the extreme antiquity of the human race may thus require revision, if it be not rather placed in the same category with the exploded evidence of the celebrated "Abbeville jaw," for the conclusive confutation of which we were indebted at the time to Dr. Carpenter.

If you can afford space for this letter, the message of this new aerial visitor may have some interest for others, as it has had for me.

I am, sir, your faithful servant,

W. B. GALLOWAY.

1, Fitzroy Road, N.W., Dec. 4.

As was to be expected, my letter was treated with contempt, as it made nothing in favour of the accepted theory, but came even under the grave, if not fatal, suspicion of favouring "a theological theory," that is, the Bible! But no attempt was made to invalidate Mr. Smith's statement of fact or to impute deception. On the other hand, there are detailed circumstances described which seem to preclude the assumption of *mistake*. The stone when found had "a piece of the upper part seemingly blown off, which showed the interior to be a pure flint of a brownish tinge. Lying on this was a fine powder in crystals, like silica; others more needle-shaped." These cannot but recall to mind the crystals of sublimated silica described by Dr. MacCulloch as casually obtained by him in an experiment¹. And it is added, "Then also metallic grains are visible—nickel, I suppose." The fall had been witnessed by four

¹ See above, p. 58.

different people; and the stone, when found, had these marked peculiarities, though such as might afterwards be easily effaced. Accepting it as a yet uncontradicted fact, it is in harmony with many of the features of the chalk and flint formation; and it is manifest that it cannot be assumed to be the only one of the kind; therefore, however disagreeable to some, it necessarily suggests some change or modification of previous theories.

I know not that there is much reason for reluctance to part with these, even though the parting with them may involve the surrender of some fine-spun theories as to the "flint implements" in the drift, which have been found in such prodigious multitudes as to present, even to M. Boucher de Perthes, himself an enthusiastic collector of them as human works, and indeed the originator of that new rage, a truer suggestion of their origin, when, in a moment of lucid perception, he says, "One would have thought a shower of them had fallen from the sky." The hopeless imbecility of the evidence by which these flint flakes and fragments have been sought by some to be converted into proofs of human life and handiwork in long pre-Adamite ages, has been ably and convincingly exposed in a pamphlet by Nicholas Whitley, Esq., one of the honorary Secretaries of the Royal Institution of Cornwall^{*}. But that question (if question, or even dogma, it remain to some "professing themselves to be wise") must be held subordinate to the greater and more comprehensive subject now before us. For even if genuine and conclusive traces of man could be produced from the

^{*} "The 'Flint Implements' from the Drift not authentic."—Longman & Co., 1865.

drift, the question would return, how far these traces should not affect previous opinions as to the age of the drift; for those opinions may be erroneous, as easily as some former ones affecting the granite.

The great extent of the chalk and flint formation in England, France, various parts of Germany, Sweden, Russia, and other parts of Europe, from Denmark to the Crimea, and to Palestine and Egypt, not to go farther, indicates a cause not operating in limited spots, such as hot springs, but of wide extension, yet of definite boundaries; and the existence of thermal springs in all the localities is quite hypothetical. Further, since the formation in question is supposed by them to have taken place at the bottom of the sea, the thermal waters, if issuing from springs at the bottom, being in that case balanced by the column of water above, would not flow (unless derived from high neighbouring lands), except by the gradual ascent of the hot particles, or else in occasional jets, forcibly expelled by the action of steam in subterranean caverns, similar to what is supposed to happen in the Geysers. But it would not, even then, rise with equal facility under the pressure of the sea; and these occasional jets, mixing with the sea, must either rapidly have cooled, and precipitated their silica in a scattered and diffused manner, giving little time for the gradual processes supposed, unless it were in the case of substances lying at the very orifice of the spring; or else there must have been, not thermal springs, but a thermal *ocean* as far north as Sweden. It cannot be shown that, in the vast majority of flints, there is any traceable organic nucleus of the particular form of the flint, round which it had been shaped by gradual accretion from silicated water. On the other hand, as I have

expressed in my letter, silica, if melted in free space, would naturally tend (unless the quantity were such as to pour it in large splashes or bucketsful, so to speak) to take there globular or rounded forms, as we see in immense multitudes of the smaller flints; but this would frequently be modified by several occasionally running together; by falling, if on land, either flattened, or moulded upon uneven surfaces and among stones; and, where the substance reached the ground in a liquid or semi-fluid state, by running over the edge of banks, and sometimes hanging pendulous there, so as to imitate the stalactitic form; by consuming with its liquid fire vegetable substances, into the underground portion of which it may have substituted itself just as in a mould formed in the sand or clay, and so taken the shape of stalks and roots; or by dropping in a soft state into undulating water, by striking casually upon marine rocks, imbedding and breaking off corals, incorporating sponges, receiving the impression of shells; or, where the flint mass was large, and therefore, in its general form, less under the control of its own molecular attraction as compared with other forces, it might be capriciously modified, even in the air while falling, by various circumstances, as of wind, or by obliquely crossing currents of the shower, by the action of electric forces, as well as by the nature of the surface on which it fell, and sometimes even flowed, as there appears evidence. Very diversified forms, often with the rounded shape much battered in or altogether lost, would be the natural result, but still leaving a predominance of it in the aggregate.

With regard to electric forces, the electric discharges frequently observed to precede the fall of

meteoric stones are well-known, and show that the meteoric substance in its fall had been, or had become by the friction of the air, intensely charged with electricity, which it suddenly discharged in passing into some atmospheric stratum of a different electric state. In its farther progress, the meteoric body, or its exploded parts, might be again electrified through its intense velocity, and might again discharge. Repeated discharges of this nature, sometimes coming singly like artillery, sometimes resembling volleys of musketry, have been observed in connexion with the fall of meteorolites. Where the mass had been solid, it is often shivered by the discharge into many fragments. To take one or two well-known cases: in a remarkable example in France in the year 1790, on the 24th of July, a luminous ball was seen traversing the atmosphere with great rapidity, leaving behind it a train of light; a loud explosion was then heard, accompanied with sparks which flew off in all directions: this was followed by a shower of stones over a considerable tract of ground, at various distances from each other, and of different sizes⁷. In 1803, on the 26th of April, about one in the afternoon, there was observed in various towns and villages of Normandy a fiery globe of great brilliancy, moving in the atmosphere with great rapidity. Some moments after there was heard at L'Aigle, and in the environs to the extent of more than thirty leagues in every direction, a violent explosion, which lasted five or six minutes. At first there were three or four reports like those of a cannon, followed by a kind of discharge which resembled the firing of musketry; after which,

⁷ Milner's "Gallery of Nature," p. 133.

there was heard a rumbling like the beating of a drum. Perhaps the first explosion signified the bursting of the main globe of the meteor; those which followed being the successive bursting of its parts, these becoming more and more comminuted, until there fell from this one globe of fire nearly 3000 fragments, the largest weighing $17\frac{1}{2}$ pounds^a. Of the usual form of these fragments, Baron Humboldt observes, "That which falls from meteoric masses, even where the internal composition is chemically different, exhibits almost always the peculiar character of a fragment, *being of a prismatic or truncated pyramidal form*, with broad, somewhat curved faces, and rounded angles. But whence comes this form, which was first recognized by Schreiber as characteristic of the severed part of a rotating planetary body^b?"

The mass, then, of any one of these satellite or planetary islets, while entire in its orbit in upper space, is not to be judged by the size of the fallen fragments; nor can we judge it impossible that, like islets of the ocean, some of them may have their mollusca and their echinites, or other of the forms of life, similar, yet different to those of this planet. "The volume of some of those passing bodies," observes Mr. Milner, "is very great. One which travelled within twenty-five miles of the surface, and cast down a fragment, was supposed to weigh upwards of half a million of tons. But for its great velocity the whole mass would have been precipitated to the earth."

Portions also of an attendant atmosphere, or meteoric gasses, sometimes reach us, differing from, and

^a Ibid., p. 135.

^b "Cosmos," i. p. 104.

not mixing at once with our air, but floating in it like large soap bubbles. A singular instance of this appears in the following account which was furnished to me in a letter by an eyewitness, and confirmed to me orally by three other eyewitnesses, who were together at the same time. I can personally vouch for its authenticity, as the witnesses were my own sisters and my brother-in-law; the place of the occurrence was at Wolverhampton. The account of it was communicated soon after by me to a member of one of the scientific societies of London, and is as follows:—"The evening of the 21st of this month (June, 1851) being very beautiful, and the sky unclouded, Eliza and I accompanied Mr. and Mrs. Graham, between six and seven o'clock, on a walk to see the fine new cemetery of this place, which is distant only about half a mile from this house. Soon after entering it we heard what we supposed to be the firing of musketry, or rather artillery; sometimes we thought it might be thunder, but we had never before heard any peals like it, following in such seemingly measured distances of time, in such rapid succession, and producing a sound so very like the discharge of a number of canon-balls, all rattling through the air for some time afterwards. This strange sort of loud firing continued to go on for about a quarter of an hour, or perhaps more, during which time the heat of the atmosphere became *exceedingly great*, but the sky remained unclouded, and beautifully serene. On leaving the cemetery our walk lay for some time through a green field, enclosed partly by a fine hedge and trees. At our left the sun was setting in majestic splendour; but, while admiring it, we were attracted by the glare of lightning proceeding from the north-east, and on looking in that direction we pre-

sently saw tremendous flashes descend to the earth in rapid succession, and then rise again as if concentrated into the forms of sky rockets, or Roman candles, and, like them, they ascended perpendicularly to a considerable height with great brilliancy, after which they branched off in forked directions. The appearance of the lightning was very awful and unusual, but not so much so as the still more wonderful phenomenon which next attracted our attention. From the direction in which the sun was setting successive clusters of blue, green, and purple balls came forward to us in the air, grouped like wild geese, about the same height above the earth, and moving at about the same rate, some perhaps quicker and some slower. A number of the balls descended perpendicularly to the grass near us, not grouped but successively, and softly, as if they were composed of a light substance. Some of these remained stationary, others moved beautifully along the grass. Two blue ones in particular fixed my attention for some time, as they advanced side by side to me, but I was prevented from observing what finally became of them by my sister Eliza calling upon me to observe how singularly some of those in the air were dissolving gradually into fleecy clouds. Some of the balls might be about the size of cocoa nuts, others about that of oranges. Mr. and Mrs. Graham and Eliza saw what I have now described, but they did not see what I called upon them next to observe, something in the distance, nearer to the sun, and much higher up in the air than the other balls, resembling a multitude of them together, contained and swayed hither and thither, as if in some invisible sheet. This moved forward, and then retreated like the waves of the sea when the tide is ebbing. The size of the balls in

this was much larger than that of the others, and the colour varying from reddish purple to red. In vain I called upon my friends to look at this wonderful assemblage. Mrs. Graham had by this time become so much afraid that no entreaties could induce her to remain longer. We had all, therefore, to return home, in order to remove her apprehensions for our safety, and thus reluctantly to leave the mighty sheet still heaving in the air. I need not tell you that all felt much impressed with what we had seen of the power and glory of God, who governs the whole elements of nature, with all its varied phenomena. Perhaps the supposed firing of artillery which we heard when in the cemetery might be the shooting of these balls from some thunder-cloud. I could have touched some of those which fell near me had not an instinctive fear prevented me."

I have ventured to give this account at full length, because it may perhaps have some bearing on the question of the existence of an atmosphere or gaseous substance, attending or enveloping some meteoric falls. And as the description is very circumstantial, it may possibly now or hereafter serve to illustrate some facts of a similar class. An atmosphere of some sort seems to us one of the essentials of the development of life, even in its lowest forms; and to this extent only does the narration bear upon the question which we have at present in hand. The general phenomena of the fall of meteoric stones have, however, to do with that question; and this is so far a fact of the same general class with those previously named, to which we may now recur for comparison.

There are many points of similarity to meteoric phenomena in the indications of the flints. In their

case the substance of the flint is to be regarded as a fused and separated portion of the larger original meteoric mass, or asteroid ; but the same principles apply. By the electrical discharge a certain refrigeration is presently produced, as is observable in hail and ice-showers attending thunder ; and where the flint mass had thus become solid, it has often been shivered into fragments by a subsequent discharge, or has had portions blown off with a clean cut ; where it has been soft or semi-fluid, fantastic forms may result. Both cases are illustrated in the actual forms of flints. A clean cleavage, or slice as with a knife, is sometimes shown, with a white film on the surface from the instantaneous passage of the intense electric fire. For flint, if exposed to the action of a hot fire, becomes white. In other cases, where the mass had been soft, the electric discharge taking place from it at several points, has apparently produced an effect at these points like the drawing out of fingers of the substance, or sometimes less drawn out, as cones thicker at the base and shorter, resembling the incipient growth of horns on the head of a young bullock ; and these projections, when of the longer, or *finger* form, have become often somewhat bent, most likely by the resistance of the air in the farther progress of the fall. Sometimes, again, the finger seems to have been drawn quite off and detached, presenting a separate flint of that form.

It is obvious that the globular or oval form would be best preserved in the small flints, which is, in fact, the case ; and that their fall would account for the small indented circle remarked in them by Mr. Phillips, and for the flattened conical shape in other instances which he mentions as having, in the locality he was speaking of, always an oval indented on the

flattened base (having fallen perhaps upon a hardened oval flint), or as having received upon the base the impression of a sponge, on which, no doubt, the soft flint had fallen without incorporating it. In other localities the round or oval flints are usually flattened, as if by falling, but without any oval indentations as the general rule. Some indication appears on many of them of having been partly smoothed by rolling, but generally with conclusive evidence from their crust, on breaking them, that they were originally of the round or oval form, which has been smoothed, but not imparted, by rolling: while other flints lying in the same bed have evidently not been smoothed or worn at all by rolling. Many others, again, have a pear shape, as of a large drop. Something as to shape would depend on whether the falling flint rotated vertically or horizontally. And, in regard to the flints which I have mentioned and partly described as found near Dunstable, to which I have noticed others somewhat though not altogether similar in the chalk hills between Croydon and Guildford, I see no way in which it can be explained how, consistently with the other particulars named, and especially with the presence of pure white chalk in their crust, and disseminated in their body, they could, as in many cases, incorporate ferruginous earth and gravel in their interior, except by falling, much flattened and spread out, while in a soft, gelatinous state, upon a precipitous bank of such ferruginous earth and gravel, with some of which adhering to them they had then, in rolling off the bank, doubled over upon themselves, like soft dough kneading a portion of the sand and gravel between the folds, and had thence fallen into water upon a chalky bottom. Most likely the precipitous

bank itself, with numerous smaller flints, had afterwards fallen in, and the whole had become mixed. But if this be not the true explanation, it may suggest the necessity of one to some other person.

It appears to me, on examination of many specimens that the pure white chalk which is often mixed in the body of the flint itself, must in these cases have been incorporated with it while in the atmosphere in the course of its fall, quite before it came in contact with the ferruginous earth; the flint and chalk being, indeed, the resolved elements of the falling substance, separated by the intense heat; and of which the light chalky and magnesian dust seems often to have been borne by the wind, and deposited, like a snow drift, in a hollow behind the shelter of some high ground, at some distance from the place where the heavier substance of the flint came down. This is much the case at Dunstable, as well as also at Betchworth and other places in the range of the chalk hills between Croydon and Guildford; and it may be well illustrated in the instance of the meteoric fall described in the letter of Mr. A. T. Smith above cited, which thus receives reciprocal evidence from its undesigned accordance with these facts.

There is no possible objection in the fact that flints may sometimes be found covered on all sides with such things as grew at the bottom of the sea, having the impress of sea weeds, corallines, small and large shells, echinoderms, and others such. In falling into the sea, or into a salt pool, or other water, the resistance and specific gravity of the water would much lighten the fall of the soft flint, even in shallow places.

There is much that is unexplained in the wonderful processes of nature; but I am not satisfied that

nature has no processes by which the finest calcareous organisms might leave their delicate traces in molten silica. The substance of delicate shells will bear a high degree of heat without the form being destroyed; and the diminished weight of the soft flint in water, as well as the greater resisting medium in its fall there occurring, would lessen the liability to breakage of shells, while the very sharpness of some of the delicate spicula or spines would make them penetrate instead of breaking.

Nor are we in a position to deny that some of the mollusca, galerites, ananchytes, holasters, echinites, and other vestiges of the lower extinct forms of life, in the chalk and flint, may be referable, not to this earth, but to those planetary isles of the upper spaces, from which the materials of the chalk and flint have been precipitated¹. This, Cuvier tells us, was thought by M. M. de Marschall to be largely the case; and if that writer even pressed the idea too far in his theory, it cannot be therefore argued to have necessarily any inherent improbability as applied to some of the facts. Does not the fall of carbon meteorolites indicate the probable existence of vegetable growth in these planetary isles? There are, as has been already mentioned, several specimens of carbon meteorolites in the British Museum; and the long line of sparks left behind in the train of some shooting stars may perhaps indicate a vast amount of burning carbon consumed to a light substance, and therefore an enormous quantity of vegetable matter in conflagration. Would not he be thought unreasonable who should deny the vegetable origin of coal? and why should not the inference be ad-

¹ See Appendix B.

mitted to have in some degree the same force in regard to meteoric carbon? But if there be *vegetable* life, why not also some low forms of *animal* life, such as on this planet abound on the most barren rocks of the sea? Shell-fish no doubt imply water, but vegetable life also implies water; and while both seem to show the existence of an atmosphere in these small planetoids, the very ignition of them, often at so vast a height above the utmost computed limits of our air², implies that they have their own atmosphere to burn in, and *that* atmosphere intensely vital, if we may judge by the brightness of their burning³.

Portions of tabular flint are to be met with occasionally, where the molten silica would seem to have fallen in a fluid state and spread itself upon a flat surface. I have found a piece of tabular flint which has received a falling elongated flint-drop perpendicularly into it, which has penetrated the upper surface, raising the edges of the chalky crust about it, as it plunged into it while not quite hardened, and has pushed out the lower surface of the tabular horizontal flint by the dint and force of its fall, while it united

² Baron Humboldt says that the heights of shooting stars, that is to say, the heights of the points at which they begin and cease to be visible, vary from 16 to 140 miles. Some have appeared to have an elevation of 183 to 240 or even 400 miles. The utmost limit of our atmosphere in height is not estimated to exceed 50 miles, and in its upper portions it is of extreme tenuity.

³ "Recalling to mind the remarkable interview between Newton and Conduit at Kensington, I would ask why the elementary substances that compose one group of cosmical bodies, or one planetary system, may not in a great measure be identical?"—Humboldt, "Cosmos," i. 120. ["Sir Isaac Newton said he took all the planets to be composed of the same matter with the earth, viz., earth, water, and stone, but variously concocted."—Cited, Ibid.]

itself with it, and, so cooling, presents in perpetuity on one side the record of its own meteoric formation; while a subsequent vertical slice has been cut off on the other side by some natural cause, perhaps electrical, showing the horizontal and the vertical flint to have become united together. The tabular form of flint is, however, by no means equally common with other shapes, though found in occasional specimens.

With regard to the tabular flint occurring in the chalk between Ramsgate and Broadstairs, while it is remarkable in extent and thickness, it is in agreement with the same general theory of its origin, and is utterly inconsistent with the notion of the flint having been formed by deposit from silicated water at the bottom of the sea. It is now some years since I visited the formation in that locality, and, not to rely on my own examination of it, I prefer to make my statements, where I can, on the authority of others who espouse the common theory, and who have been accustomed to reject the view which I believe is demonstrable from facts; and I have been informed by a very competent and self-assured writer, whose statement I accept, that the flint is frequently to be seen running down the cleavage of the chalk, in thickness varying from an inch and a half to the thinness of Bristol board. It stands patent and confessed, then, that the flint there existing was not deposited by thermal waters while the chalk lay as soft mud at the bottom of the sea; but if the chalk was formed at the bottom of the sea, as they suppose, then the running of the flint into these clefts must have been after the upheaval of the chalk with all the flints contained in it, and when it had dried and hardened, and after the formation of those fissures

into which the fluid matter of the flint has run. The chalk may doubtless have been acted on by subterranean fires or shocks, and split in clefts, through which the subterranean heat might remelt the silica of the flints, and run it into the fissure, or it might melt them into an extensive tabular cake. That the flint character should be retained in such re-melting may be remarkable; but it might be owing to this taking place under superincumbent pressure, and enclosed on all sides, to the exclusion of air. But if it be thought that this re-melting could not be supposed, then what does it show, but that the fall of the fluid flint was, in this case, posterior to the elevation of the chalk, and not under water? Else why those perpendicular fissures and veins? I make no objection to that conclusion. There is, however, a thick bed of flints at Dover, described by Mr. Williams in a paper already cited, so hard welded together, that, in the extension of some of the works of the castle, the roof of the chambers cut into it was left without any other support than the bed of flints. That seems to suggest an intermediate stage in the same process. A more intense degree of heat than that which there welded the bed together, would have fused and run the flints into one great tabular cake.

The rapid cooling of a nodule of molten silica by falling into water would, I apprehend, render it more brittle, but would not necessarily split it. Cleavages might more frequently occur independently of that, even in the air from electrical discharges. One feature in the description of the Fawley meteorolite was, that it had a piece apparently blown off; and the occurrence in flint of a natural cleavage, which cuts off part of the external crust, together with part of the inner substance of the flint, and exposes the

interior, is familiar enough, the surface of the cleavage having, in very many cases, a white porcelaneous film upon it, as if from the action of an intense but instantaneous heat, not penetrating beyond the surface.

We are not to imagine that what has happened in the breaking and splintering of some meteorolites must necessarily have taken place in all. On the granite of the Scilly Isles, and on the stony regions of Sinai and Arabia Petræa those splinters of flint are said to abound. It is conceivable that in some instances the meteorolite might be sufficiently hard to break from electrical discharges in the air, or on contact with the rock; but, in other cases, it might be soft, and even fluid, like that which, in Egypt, ran as liquid fire upon the ground. Flints, we are informed, abound upon the deserts of Egypt, and part of them may be the relics of that memorable storm of hail and liquid fire which smote all that was in the field, both man and beast, and every herb in the field, and brake every tree of the field, and in which the liquid fire that "ran along the ground" may have eaten into many of the roots of trees, and into bamboos covered and filled with superficial soil, and may have substituted and moulded itself into their forms. Specimens of similar formation are found in the flint near Dover, and have been mentioned above, and I have also found some of a similar character near Dunstable. But fossil wood covers the desert on the east side of Cairo, in such quantity that it has been spoken of as a petrified forest; where "the dreary and wide expanse, treeless, and almost shrubless, excites, by the remarkable contrast of the present with what is apparently the not far distant past, the wonder of

the most careless observer⁴." The fossil wood is said to be converted into chalcedony, a silicious mineral allied to flint. And the area covered is said to be four miles east and west, and three miles and a half north and south. Whether this singular petrified wood stands thus connected with that storm of judgment, I leave for consideration; but certainly on that occasion the fluid fire was mingled with aqueous hail⁵; and the combination of the two, if from one source, would favour the idea of *water* also existing in many of these miniature worlds; the probability of which appears, as already remarked, on other accounts. But passing from this, which we here notice only incidentally, and returning to the breaking and splintering of many meteorolites, I affirm that there is a tendency in hard flint when it has been gradually exposed to a red heat, and is then allowed suddenly to cool, to show, in splitting, natural pyramidal forms, such as Humboldt remarks of other meteoric stones⁶, and also sharp-edged slices, which might afterwards furnish natural knives and spear and arrow-heads, to such as chose to take them. These, though convenient for man so to use, ought not to be attributed to human design and manufacture. Those frequent smaller chipped or scaled-off surfaces which are sometimes observable on these flints, if not produced from the same primary natural causes (as to which it would only be a question of more or fewer, there being some similar appearances on flints which are undoubtedly

⁴ Geological Magazine, vol. vii. p. 306, Paper on the "Petrified Forest near Cairo," by W. Carruthers, F.L.S., F.G.S.

⁵ The description in the book of Exodus is evidently so understood by the ancient author of the Apocryphal book of Wisdom, chap. xix. ver. 20, and correctly so.

⁶ "Cosmos," vol. i. p. 104, quoted above, p. 70.

natural), might be the effect of scaling off in the process of rapid cooling, or of the subsequent alternation of action of heat, moisture, frost, and possibly even collisions. The small chipped surfaces, it may be noticed, are not generally in direct planes or straight lines, but curved a little inward, and slightly scooped, unlike the straight cut of gun-flints. I have remarked many small indentations, not very dissimilar, on undoubted meteorolites, in the collection in the British Museum; such as, for instance, on one which fell at Pultusk, in Poland, on January 30, 1868, and also on some models of large meteorolites. The difference of these is that the edges of the indentations are not so sharply defined, and the spherical curvature of the depression is scooped rather deeper. If the resemblances to supposed forms of human design were found in the flints all ready classed in their actual sites where found, just as they are presented in palæolithic collections set all out together, apart from other casual forms; or if they were found at least with some evidence of human purpose and arrangement, as a workman puts aside by themselves his finished tools, and does not throw them back into the rubbish heap, when they have been brought with great labour time and skill to the utmost finish and perfection he can impart, then the conclusion attempted to be drawn from them might have more plausibility; but when each of these has only recently been picked out from a thousand other casual forms, all mixed and partly similar yet unsuitable, and has been set out to the best advantage to suit an archaeological idea, the evidence of design is to be recognized in the archæologist only: and if it is attempted to be balanced against authentic history, and, still more, against Holy Scripture, it indicates the natural

incapacity of a man, with an archæological hobby, for estimating the relative value of evidence'. Of course, we know that the casual forms of nature sometimes resemble works of human design.

Sir John Lubbock mentions very particularly the fine porcelaneous polish and gloss of surface which he found upon the more ancient of what he is pleased to consider flint implements of human manufacture. That fine porcelaneous surface is an evidence of natural, not artificial, production, and is, I think, the effect of the fire of an electrical discharge in the moment preceding the meteorolite's fall, or of intense remaining heat in cooling, probably not under water. It is not found, I think, that lying in the earth for ages produces a fine porcelaneous polish upon any thing; but it would more readily destroy the polish, even if those ancient savages, who are assumed to have made the implements with the porcelain finish, were supposed by Sir John to be the remote ancestors of that ingenious people the Chinese, and that we have here the Darwinian germ of their beautiful art; which I do not find him any where affirm.

Accompanying the fall of the flint meteorolite, as described in Mr. Smith's letter, another magnificent meteor was observed to fall perpendicularly, almost at the same time, about a mile distant from the other, drawing a long line of sparks, bursting not far from the ground with a bright flash, depositing, as far as known, no stone, but dispersing in a luminous cloud which continued visible full half an hour, assuming

' Sir Walter Scott, himself a man of great antiquarian knowledge and genius, has finely satirized this in his "Antiquary," in the scene with old Edie Ochiltree at the Roman camp.

various shapes as it was carried in the wind. Was this cloud incandescent chalk dust? And, as chalk is found to have in combination with it a proportion of magnesia, (some of the foreign chalks as much as from 8 to 10 per cent., and the green sand in England 3·8%,) was the intense light a natural exhibition, on a grand scale, of the brilliant light of burning magnesium, the dense smoke emitted from which would in that case mingle with the cloud of incandescent chalk dust? We cannot positively answer these questions; nevertheless the circumstances, in combination with the fall of the meteoric flint, are suggestive, and were not arranged to suit a theory.

It is impossible to regard the recorded fact as less than a natural, and, if correctly recorded, a conclusive testimony, to the true origin of the chalk and flint formation. On observing the manner in which flints are scattered over every square yard of a large portion of country, visible on the surface of every ploughed field for many miles, between London and Dunstable, or in another direction near Boxhill, Walton-on-the-Hill, Dorking, Betchworth, and other parts of that district, and on examining the flints there found, with the chalk drifts separate from them, no other conclusion appears to me so natural, and so agreeable to the facts, as that the chalk and flint are of meteoric origin*. Again in other parts, in its successive layers of flint upon the chalk itself, that formation contains evidence of several successive occurrences of the meteorolite storm of "hail stones, and coals of fire;"—"fire which was mingled with the

* Milner's "Gallery of Nature," p. 733.

* I have in my possession flints gathered by myself which appear to me to show by their form and structure distinct evidence of the truth of the opinion which I have expressed.

hail," and which, as in one memorable recorded instance, even like molten silica, "ran along the ground¹." It opens to us a grand and awful epoch in the history of the world, when it is probable that, not only upon the sea, but also to some extent upon the land, the fiery shower of hot dust and stones fell like a withering blight. We know not whether some experience of that blight had not recently occurred before the birth of the patriarch Noah, and given a fresh and feeling appropriateness to his father's hope, expressed in *naming* him, that he should COMFORT their hearts concerning their work and toil of their hands, *because of the ground which the Lord had cursed*. For though there is, doubtless, an allusion here to the original curse pronounced at the fall of man, the fresh mention of it thus at the period of Noah's birth, in connexion with their personal difficulties and toils, seems to suggest some recent exemplification of it. Be that as it may, and be the period of those great successive meteoric falls what it might, a considerable mass of heavy materials was added to a portion of the surface of the earth, and the natural re-adjustment of exact equilibrium was most probably made by a flow of the waters of the sea towards another part. To effect equilibrium, the bulk of water moved for compensation must have been more than twice the bulk of the solid matter heaped upon the surface, or dropped into the sea. Consequently a receding of the sea, and a relative rise of the chalk, might be expected to take place; and it took place accordingly. The change, even if it were gradual, was probably not unaccompanied with earthquakes; and it is remarkable that Baron Humboldt has placed the

¹ The word there used in the original for "*ran*" is one which is often applied to *flowing*, as of water, or of honey.

phenomena of earthquakes in conjunction with those of great meteoric showers¹. A series of causes was thus put in operation the ultimate consequences of which it would be impossible for man to foresee, but it may nevertheless be possible partly to retrace.

An amusing illustration of the manner in which a seemingly inadequate cause may put in operation a chain of causes eventuating in a considerable result, presents itself to my recollection in the ingenious device of one of my fellow students, many years ago, to supply temporarily the want of an alarum clock, on some occasions when he was desirous to commence study at an unusually early hour. He mentioned it to me at the time, and, at my request, showed me it in operation. Opening his watch-glass, and passing the loop of a fine silk thread over the hour-hand to rest on its projecting arrow-head, having a small leaden drop attached to the other end of the thread, he laid the open watch horizontally, on a raised stand, at such an angle to the thread, that when the hour-hand came round to the hour desired, the silk thread would slip off the pointer, from the weight of the small leaden drop. A little below he balanced a light teaspoon in such a position that the lead drop, when liberated, would fall into the bowl of it; a little below that he balanced in the same way a large heavy spoon; and again a little under it a somewhat heavy book projecting half way over the edge of the table, so as to be easily upset. Beneath the edge of the projecting book a chair was then propped obliquely, in such a position of unstable equilibrium, that when the silk thread slipped from the hour-hand, and the lead-drop fell into the bowl of the tea-

¹ "Cosmos," vol. i. p. 113 note, p. 114 note.

spoon, that fell upon the heavier one below it, and that upon the book edge, which was thus upset upon the chair, which in turn fell heavily against the door with a great noise, effectually rousing the student to his work at the time he had appointed; and all from the silent and imperceptible motion of a watch's little hour-hand, and the angular position of a fine silken thread. Even so at the appointed day and year, through a chain of causes put in operation by a great meteoric fall of matter upon a portion of the earth's surface, an ultimate effect, much greater than that which was attributable directly to the great weight of the meteoric matter deposited, was silently prepared, and in the end suddenly produced.

PROPOSITION XIV.

THE rapid and revolutionary progress of Geology in the present century has strikingly shown its unsteadiness and unrelia- bleness hitherto as a department of science.

Not to go back to that long series of exploded theories which Sir Charles Lyell has given a list of, it is fresh in the memory of those who have given any attention to the state of the science, that the unstratified granite was received as the oldest primary rock. It was the common theory that it was a solidified portion of the supposed melted and fluid mass of which the body of the globe consisted, which was thus cooled and crusted over to a thickness of many miles, but still a small depth in comparison with the diameter of the globe. At the depth of about one hundred miles below the surface of this planet it was thought that there existed a general mass of incandescent materials, and it was not decided whether lavas of different kinds were not simply ebullitions of some portion of that inner incandescent and fluid mass¹.

All this is now changed. Sir Charles Lyell tells us that the granite, so far from being distinctively a

¹ Such was the favourite theory in Buckland's day (1836), and which he embraced. See his *Bridgewater Treatise*, vol. i. p. 48, and ch. vi.

primary rock, is of different ages, and sometimes of comparatively recent origin, more recent than the chalk². And the theory of an incandescent state of the planet, which was felt not to be reconcilable with the supposition of a gradual spontaneous and progressive development of the various species of animals in millions of years from fish or mollusca, and of the present race of philosophers from the remote pater-nity of the orangoutang, has therefore been put aside, no doubt with reason. In opposition to the ebullitions of the quick-pot-boiling theory (as it may be called) of a fluid incandescent world bubbling up through a superficial crust, we have got what may be considered, for distinction's sake, the slow-pot-boiling theory, according to which nothing is to be supposed to have gone on any faster in the past than it is going on in the present. If the land somewhere has risen $2\frac{1}{2}$ feet in a century³, that is the sort of measure to apply to the rate of the elevation of the Alps. Indeed, so slowly is the pot to boil, that it may be doubtful whether it boils at all, or whether a slow and gradual pressure and a casual earthquake will not explain all past and present changes with the help of the varying eccentricity of the earth, and the millions of ages, which, in spite of a protest from Cuvier, nothing can be easier than for old men and children, young men and maidens, to "call into existence by a dash of the pen." The eternity *a parte ante* gives ample room for them.

Every theory, however, if it lead to investigation and inquiry, brings some facts to light. Burnett

² "Principles of Geology," vol. i. pp. 70, 143, 253.

³ Lyell, "Antiquity of Man," pp. 282, 285; and Sir John Lubbock, "Pre-historic Times," p. 406.

no doubt was absurd in supposing that the crust of the earth was formed in the manner of a sort of oil-cake floating on the waters, but his work is not without value in some points. Lyell and Scott Moore in turn may exhibit a profound *naïveté* in assigning the slow and imperceptible coming on of a glacial period, supposed to result in some hundred thousand years from the varying eccentricity of the earth's orbit, as the means of freezing up living elephants in their pastures in ice 200 feet thick, or, at least, all of a sudden so thick that not a drop of water could percolate to the intombed carcase. There may be something of an amusing weakness and presumption in bringing the transient guesses of a science in such a state of flux to contradict the records of man in the Scriptures of truth. And the popular acceptance of their specious hypotheses as science may illustrate the existence of that faculty, which, the late Dr. Chalmers is said to have remarked, had been overlooked in the philosophical enumeration of the faculties of the human mind, "the faculty of gullibility." Many are awed into it by the assurance with which these gentlemen inform us that it is only prejudice, such prejudice as withstood the illustrious Galileo, which now withstands the acceptance of their theories; and so they give us chapters upon the "prejudices;" always comparing themselves to Galileo! No man likes to be thought prejudiced like the persecutors of that great philosopher; therefore the two hundred and fifty thousand years of the mammoth and of man must be swallowed by the would-be-scientific, large as is the mouthful. But the facts of nature which are brought together, even by those who work from some wrong theory, and though it were with some wrong animus, are not to be disregarded.

With respect to the interior of the earth, it was inferred from astronomical calculation by Mr. Hopkins that the recession of the equinoctial points would be greater than it is if the earth were not solid for 400, nay, more probably, at least from 800 to 1000 miles deep. Sir Charles Lyell observes that this calculation is consistent with a much greater depth of solidity, and that it does not preclude us from imagining that lakes or even seas of melted matter may be distributed through a shell 400 or 800 miles thick, provided they be so enclosed as to move with it, whatever motion of rotation may be communicated by the disturbing forces of the sun and moon⁴. The late Mr. Denison⁵ popularly illustrates the principle on which the calculation depends in the following manner:—The sun and moon would twist the axis of the earth rather more if the protuberance under the equator were fluid; which may be roughly illustrated thus:—A pendulum with a bob made of a glass globe filled with quicksilver swings rather faster than a pendulum similar and equal in all respects except in the bob being solid. The reason is that the hollow globe does not stick to the quicksilver and hold it fast, but slides round it as it turns a little in swinging, and so the mercury does not turn with it; whereas the whole of a solid bob has to be turned as well as swung at every vibration, which uses up more of the force of gravity, on which the quickness of the pendulum depends.”

But to proceed to another point. Facts have been brought to light by artesian wells which give us

⁴ “Principles of Geology,” vol. ii. p. 204. The Philosophical Transactions of 1839 are quoted.

⁵ “Astronomy without Mathematics,” p. 45.—Christian Knowledge Society.

reason to know that there is, even at present, going on a process of interposition of strata at considerable depths; and this must in some cases introduce error into those conclusions which are based on the supposition of the underlying strata being necessarily anterior in time to those which are found apparently superimposed. In the crust of the earth there are extensive subterranean passages and caverns, and into and through these it appears that sand and shells and fish, even *living* fish, are carried from great distances. The facts which have come to light in the opening of some artesian wells reveal what must be proceeding unseen in innumerable cases where no borings have been made. "At St. Ouen, in France," says Sir Charles Lyell, "five distinct sheets of water were intersected in a well, and from each of these a supply obtained. In the third water-bearing stratum, at the depth of 150 feet, a cavity was found in which the borer fell suddenly about a foot, and thence the water ascended in great volume. A similar falling of the instrument several feet perpendicularly, as if in a hollow space, has been remarked in England and in other countries. At Tours, in 1830, a well was perforated quite through the chalk, when the water suddenly brought up, from a depth of 364 feet, a great quantity of fine sand, with much vegetable matter and shells. Branches of a thorn several inches long, much blackened by their stay in the water, were recognized, as also the stems of marsh plants, and some of their roots, which were still white, together with the seeds of the same in a state of preservation which showed that they had not remained more than three or four months in the water. Among the seeds were those of the marsh plant *Galium uliginosum*; and among the shells, a

fresh water species (*Planorbis marginatus*) and some land species, as *Helix rotundata*, and *H. striata*. M. Dugardin, who, with others, observed this phenomenon, supposes that the waters had flowed from some valleys of Auvergne or the Vivarais, distant about 150 miles, since the preceding autumn. An analogous phenomenon is recorded at Reimke, near Bochum in Westphalia, where the water of an artesian well brought up from the depth of 156 feet several small fish three or four inches long, the nearest streams in the country being at the distance of some leagues. In some artesian wells sunk by the French in the north-eastern part of the desert of the Sahara, small fish have frequently been brought up alive with the first gush of water from a depth of 175 feet. M. Désor informs us that in January, 1863, he saw some of these fish in a well in the Oasis of Ain-Tala. They were of the genus *Cyprinodon*, not blind, like those taken from the underground caverns of Adelsberg or Kentucky, but with perfect eyes. The nearest ponds or lakes were at a great distance on the surface of the desert, and in this and the other cases before mentioned of the subterranean transportation of shells, fish, and fragments of plants, we see evidence of the water not having been simply filtered through porous rock, but having flowed through continuous underground channels. Such examples suggest the idea that the leaky beds of rivers are often the feeders of springs." I will add to these remarks that the same may be expected to have occurred from cavernous openings under the bed of the sea, and that deposits of marine shells and fish, with coral and other productions of the sea, must thus have been carried to unsuspected places. Much more is suggested by it than a source of springs. It plainly

appears that in many places in the course of ages layers of marine, and also of fresh-water deposits, and vegetable seams, may have been interposed deep among the strata, which would constitute an inevitable source of errors in the reasonings of geologists not making allowance for that method of their introduction, but assuming that every deposit was formed upon what had then been the surface of the ground. A few thousand years of error in chronology might easily be introduced in this way; but as Sir John Lubbock has justly observed⁶, “a period of ten thousand years, long as it may appear to us, is very little in a geological point of view.” And again, Hugh Miller has remarked⁷, “There are no calculations more doubtful than those of the geologist.”

The cavernous structure of the earth, and many of the disruptions of the strata from the sinking-in of portions of the crust, appear to have been owing to the contraction of the underlying strata in the process of solidification and of cooling, or, in other cases, of drying and hardening; or else the converse effects of heat, and the subterranean disengaging of gases, lifting thereby portions of the strata, and forming hollows as in the rising of bread. The earth in many places sounds hollow even to the tread, it vibrates under the passage of a waggon, and the long rumbling sound, or sometimes the sound of rushing as of batteries of artillery at gallop, which attends earthquakes, as well as the vast distance over which the shocks are felt, indicate the prodigious extent and depth of the subterranean communications, and the cavernous structure, as well as some degree of flexibility.

⁶ “Pre-historic Times,” 2nd edit. p. 406.

⁷ “Old Red Sandstone,” ch. ii. p. 54.

A circumstance is mentioned by Sir Charles Lyell, which serves to illustrate or suggest how alternations of marine and fresh-water remains may have occurred without very long periods of time being implied. "Captain Smyth," says he, "in his survey of the coast of the Mediterranean, found the sea, opposite the mouth of the Rhone, to deepen gradually from four to forty fathoms, within a distance of six or seven miles, over which the discoloured fresh water extends; so that the inclination of the new deposits must be too slight to be appreciable in such an extent of section as a geologist usually obtains in examining ancient formations. When the wind blew from the south-west, the ships employed in the survey were obliged to quit their moorings; and when they returned, the new sandbanks in the delta were found covered over with a great abundance of marine shells. By this means we learn how occasional beds of drifted marine shells may become interstratified with fresh-water strata at a river's mouth."

In the same place an instance is given by Sir Charles which shows how very recent even some crystalline rocks may be. "That a great proportion, at least, of the new deposit in the delta of the Rhone consists of rock, and not of loose incoherent matter, is perfectly ascertained. In the museum at Montpellier is a cannon taken up from the sea near the mouth of the river, imbedded in a crystalline calcareous rock. Large masses, also, are continually taken up of an arenaceous rock, cemented by calcareous matter, including multitudes of broken shells of recent species*." Had it not been for the cannon, who can say whether we should not have had an

* Lyell, "Principles," vol. i. p. 429.

antiquity of some ten thousand years ascribed by geologists to the rock in which it was imbedded, and a calculation based on it to show the enormous ages attributable to the strata below it? We cannot say; but, as Sir John Lubbock has justly observed, "ten thousand years are very little in a geological point of view;" and therefore we could not have been entitled to reproach them with any error of that very trifling amount. But reasoning back from their alleged antiquity of the mammoth, would a tenth or twentieth part of the measure of time, which they have assigned to it have seemed extravagant for the crystalline rock? The allowance seems even more than moderate, and might suggest to Sir John the inquiry whether the cannon may not have belonged to the Bronze or Iron age of Pre-Adamite man. It would be satisfactory to find some proof that at least in respect of art and manufactures, man with his native powers and inventive intellect had not remained without progress or development for the vast periods assumed. Two hundred, or one hundred thousand years of rude unshapely flint implements, or a tithe, nay, a twentieth or a hundredth part of that time, to a whole race of beings possessed of the faculties of man, and having the range of the whole earth, are utterly inconceivable. Might we not be helped to a demonstration from Sir Charles Lyell, or some other geologist, that that cannon must have been at least 35,000 years old, and that, therefore, the invention of gunpowder must be much older? Any one, indeed, of moderate reading knows on the authority of Milton (*Par. Lost.*, b. vi. 508), who again goes partly on the still older authority of Edmund Spenser (*Faërie Queen*, b. i. canto vii. 13), that gunpowder was invented some considerable time

before Adam, which quite cuts out the claims of Roger Bacon and the still earlier Chinese. But still those are poets, and it may be agreeable to have a scientific confirmation of this from Sir Charles. The proof is patent in the estimate which he has formed of the time necessary for the Niagara to have cut its deep channel through the hard limestone rock from the escarpment of Queenstown back to the present site of the Falls, a distance of about seven miles. In his calculation he has evidently assumed that at no period within the said thirty-five thousand years could the limestone deposit have existed in a soft state saturated with water, of course putting out of the question a sludgy state such as a cannon could have sunk into. His whole reasoning is based on the supposition that it was as hard thirty-five thousand years ago as now. "Mr. Bakewell," says he, "son of the eminent geologist of that name, who visited the Niagara in 1829, made the first attempt to calculate from the observations of one who had lived forty years at the falls, and who had been the first settler there, that the cataract had during that time gone back about a yard annually. But, after the most careful inquiries which I was able to make during my visit to the spot in 1841-2, I came to the conclusion [setting aside of course the older evidence, and longer experience of the first settler, so as to multiply threefold the requisite time] that the average of one foot a year would be a much more probable conjecture. In that case it would have required 35,000 years for the retreat of the Falls from the escarpment of Queenstown to their present site⁹." If so, then, by parity of measurement of time, we may

⁹ Lyell, "Principles of Geology," vol. i. p. 361.

conclude it to have been more than thirty-five thousand years ago, (and who, in that case, shall say it was not 250,000 years ago ?) since that fossil cannon was founded ; and the invention of gunpowder must of course have been older still.

If, without invidiousness towards a veteran geologist, I may dare to breathe a dissentient thought, it might be this, that, after all, it is not proved that the limestone deposit had been of its present hardness for any thing like so long a time. Let not that suggestion be taken unkindly as if I were like one of the prejudiced persecutors of the great Galileo ! It is not my fault if that unlucky cannon should have spoilt, even in the most indirect manner, the interesting demonstration of the thirty-five thousand years of the Falls of Niagara. Neither is the cannon exclusively to blame. The fossil human skeleton found at Guadaloupe, imbedded in hard calcareous stone, and now in the British Museum, doubtless gives evidence, like the cannon, that the limestone was not always of its present hardness, and it is admitted to be of recent formation. At the Giant's Causeway in Ireland we are informed that chalk has been converted into granular marble by contiguity with basalt. The same has no doubt taken place in other parts. In describing the journey to Gadara, along the valley of the Jarmuk, in the recent publication of the Palestine Exploration Fund, "The Recovery of Jerusalem," by Captain Wilson and Captain Warren¹, the course of the Jarmuk, rushing in some places like a mountain torrent through the deep chasms in the chalk, in others lazily flowing along through small plains, where the contrast between the bright green

¹ Vol. ii. pp. 372, 373, 341. Bentley, 1871.

of the spring verdure, the brilliant white of the chalk, and the sombre hue of the basalt above it, was very pleasing, is interestingly described. At Gadara the chalk underlying the basalt is spoken of by them as "limestone" capable of a fine polish. And again, in speaking of the Lake of Galilee, the hills on either side are described as "limestone capped in places with basalt." Other causes may have operated other transformations. Sir Charles himself could furnish us with many illustrations. No doubt the change which may have passed over the calcareous deposit of the region of the Niagara may have been very great as regards its degree of hardness, even within one-half of *five* thousand years, or perhaps a much shorter period. Moreover, we have heard that the progress, or rather regress, of the Falls of Niagara in the last year or two has been of itself not quite in agreement with the calculations.

PROPOSITION XV.

IN the smelting of iron and other ores the molten metal runs down, and is drawn off at the bottom ; and, as a general rule, where there is no insuperable obstacle, the heaviest things fall to the bottom.

Simple as this principle seems, it must not be lost sight of. As many of the rocks, such as granite, porphyry, serpentine, trap, basalt, obsidian, are palpably of igneous origin, and others have been partially fused by contact with veins of these, the same effect mentioned in our proposition must necessarily have taken place on a great scale in the vast smelting apparatus of the earth. Accordingly the specific gravity or density of the whole globe is known to be more than twice that of the rocks at its surface, showing that its central parts must exceed its superficial in their specific gravity in a much higher degree. Among the metals, mercury is fluid at ordinary temperatures, and its specific gravity is almost double that of cast iron. It is heavier than all substances, even metals, except gold, platinum, and tungsten ; and therefore would sink lower : or, in other words, all other substances, except these three, would even float upon it ; but gold would sink into it, in a certain proportion would incorporate

with it, and increase its weight. It is found not only in ore, but also native; and no doubt, wherever opportunity is given, it runs down. Its particles have a greater attraction for one another than for most other bodies, and thus it tends to run together in little globular masses, and these ever into larger, joining together and keeping together. At a mean barometric pressure of 29·9 inches it does not boil at a lower temperature than 662° (Fahrenheit), which is considerably more than three times the temperature of boiling water, and 42° above that at which lead melts; and, under a deeper barometric pressure, of course the temperature of its boiling point would be much higher. It does not become solid except at -38° (or 38° below zero); and therefore its range of fluidity is very great, extending over 700 degrees. It becomes volatile at greater heats than that above mentioned.

If the interior of the globe of the earth were supposed to be in an incandescent state, gold, silver, lead, and other metals would run down towards the centre in a state of fluidity, and the gold and platinum would run lowest. If it be not in an incandescent state, still mercury would run down towards the centre, carrying with it probably portions of gold and silver, with which it has a tendency to combine; and the adhesive attraction of quicksilver for gold is greater than for any other metal¹. The cavernous structure of the earth, with its cracks and fissures, would afford many channels; and the extensive

¹ The following table, which is given in "Brooke's Elements of Natural Philosophy," represents the comparative intensity of the adhesive attraction exercised between different metallic surfaces and mercury, according to the research of Guyton and Quetelet.

existence and operation of volcanic fire would contribute to throw the metals downward. Nay, in the first drawing together of the globe's materials, whether incandescent and fluid or not, the heavier must in the natural course be supposed to have tended to the centre. It was a theory of geologists that the great mass of the globe was now in a state of fluidity from intense heat, a conclusion supported by them on the evidence of the increased degree of heat found at increased depths in mines. Sir Charles Lyell does not favour the idea of so much central heat as had been supposed, It is indeed impossible for us to say whether the increase of heat with the depth experienced in mines goes on at all in like proportion at enormously greater depths. The causes of volcanic fires appear to be not central, as if the incandescent fluid mass of the earth itself surged up through these occasional vents, but local in the earth's crust, though deep; and Sir Charles thinks

Metal Discs 1 inch in diameter.	Force of Adhesion in grains*.	Disc of Metal.	Comparative force of Adhesion.
Gold . . .	446	Gold . . .	23·63
Silver . . .	429	Silver . . .	22·74
Tin . . .	418	Tin . . .	22·15
Lead . . .	317	Lead . . .	21·04
Bismuth . . .	272	Bismuth . . .	19·71
Zinc . . .	204	Platina . . .	14·98
Copper . . .	140	Zinc . . .	10·81
Antimony . . .	126	Copper . . .	7·52
Iron . . .	115	Iron . . .	6·10
Cobalt . . .	8		

* Reference is given to Guyton-Morveau in Kastner's "Experimentalphysik," Heidelberg, 1810, and Quetelet, "Position de Physique," p. 104, Bruxelles, 1834.

that they may be connected with chemical combinations resulting from the ingress of water, through the cavernous and leaky bottom of the sea, to unknown subterranean depths. In the observations with the thermometer in deep mines the heating effect of pressure on some strata, and the heating tendency of coal in large masses, may not have been sufficiently adverted to : but the bottom of the sea indicates no increase of temperature with depth ; but, on the contrary, intense cold at the greatest depth, such as of four miles. The facts of magnetism contribute to render it improbable that the mass of the earth is in an incandescent state. “ If,” says Baron Humboldt, “ we represent to ourselves the interior of the earth as fused, and undergoing an enormous pressure, and at a temperature the amount of which we are unable to assign, we must renounce all idea of a magnetic nucleus of the earth. All magnetism is certainly not lost until we arrive at a white heat, and it is manifested when iron is at a dark red heat : however different, therefore, the modifications may be which are excited in substances in their molecular state and in the coercive force depending upon that condition in experiments of this nature, there will still remain a considerable thickness of the terrestrial stratum which might be assumed to be the seat of magnetic currents².” But, all things considered, the probability is utterly against the supposition of the earth being in a state of fusion.

Let it further be observed, that, at the centre of the earth, there is no tendency to gravitate down or up, or to one side more than to the other ; but the central body, of whatever nature, is, in this respect, left

² Humboldt, “ Cosmos,” i. 183.

freely suspended, and, if fluid, free to its own native tendencies. A large globule of quicksilver, for instance, is, at the surface of the earth, much flattened by the central attraction of the earth; but, at the centre of the earth, the largest quantity would take its own natural globular form, subject only to the oblate expansion caused by revolution on an axis, and, if it were a globular mass of very large dimensions, subject also to the tidal influence of the sun and moon. The pressure of the several overlying strata downwards must, so far as this principle is concerned, *diminish* towards the central parts of the earth; and, when they have hardened and solidified, their pressure would no longer exist, but they would be as a vast dome built close over the interior fluid nucleus, and would not interfere with its free movement except by a certain amount of friction, if the nucleus and the spherical dome did not move quite together, in which case the friction would produce heat and electric currents.

Taking the specific gravity of water as 1, that of the various rocks may be 2, $2\frac{1}{2}$, or from that to 3. Rock crystal is 2·653; but the globe of the earth, taken all together, is about $5\frac{1}{2}$ times that of water. The lighter materials of its surface must be balanced by heavier in its depth. The metals weigh as follows :—

Cast Iron	7·207
Cast Copper	8·788
Silver	10·474
Lead	11·445
Mercury	13·596
Gold	19·358
Platinum, forged	21·837
Do. laminated	22·069

There has been prevalent what I cannot but suspect

to be an exaggeration of the accumulated pressure of the strata of the earth at great depths, by which alone some have even proposed to account for the density or weight of the earth as a whole being double that of its superficial rocks. But this arises from not taking into account the arched and dome-like form and structure; for it is manifest that in such a structure it would be as erroneous to consider the pressure as all downward, as it would be to suppose that in a strong brick arch of twenty rows of bricks in thickness or depth, the lowest row was burdened with the weight of all the others; which is the reverse of the fact, the others operating to keep off the superincumbent pressure of whatever mass of weight may be heaped above. So, taking the depth at so many rows of strata, say of ten miles thickness each, or whatever may be thought best for illustration, and considering that there might be above two hundred of these, at ten miles each, before reaching the nucleus, it is manifest that the pressure of the outmost soil would be borne by the strata next it, and that towards the central parts the successive rows of arches would bear their own weight by the lateral pressure of the arch. This is supposing the earth to be solid in the main, which is most probable. But if it were fluid, the fault of overlooking the principles enunciated in this proposition is still more conspicuous.

Since, therefore, the heaviest materials fall relatively lowest, as we may see in the settling of fluid or semifluid mixtures, and also in grains of loose gold, lead, or other heavy metal, with dry sand and lighter substances shaken down together; and since the density of the globe compared with its superficial strata indicates the same fact, we shall not greatly

err in assigning the heaviest substances principally to the bottom, especially such as are not found only in single localities, but widely diffused, though, it may be, small in the quantity found within reach of the surface, and this is particularly the case with gold.

PROPOSITION XVI.

THE variations of the magnetic needle are such as are in agreement with a change of the earth's axis amounting to about $18\frac{1}{2}$ degrees at the north pole, and with a metallic nucleus of the earth, fluid in its exterior portion with solid centre of heavier substance, at a depth of about 2227·2 miles beneath the surface of the earth, having a diameter of about 3462·8 miles, subject to diurnal tides from the influence of the sun and moon, sensitive to the difference of the attractive power of the sun at the aphelion and perihelion (that is, the farthest and nearest distance), and retaining, relatively to the earth, the former axis of its rotation, its poles also performing a secular revolution round the poles of the new axis of the earth. By these causes electric or magnetic currents and other phenomena are produced; which are modified, in their effects at the surface of the earth, by the earth's rotation, and by the great depth, variety, and occasionally contorted forms and dislocations of the strata through which they are transmitted. These local modifications can only be determined by local observation, in the manner in which they have been dealt with, and are indicated, on the magnetic charts; for they arise from causes lying deep, and beyond the reach of direct investigation.

For the sake of clearness and brevity I have put this proposition in a positive form, although aware of the extreme difficulty; and I will proceed to submit, with much deference, the general facts on which I venture to rest it. I trust my readers will pardon me if I err on the side of an elementary simplicity of treatment more than may be necessary for some.

(1.) The magnetic needle varies both in its declination, or horizontal position in reference to the pole, and also in its dip, when constructed so as to indicate that vertical inclination. In 1576 the needle at London, according to Mr. Robert Norman's observations, pointed about $11^{\circ} 15'$ to the east of the true north; its easterly declination gradually diminishing¹, till in 1657 the needle at London pointed due north; from which position it did not swerve till after 1662; then, passing gradually to the westward, year after year it pointed more and more westerly, till it reached the maximum of westerly declination, which, according to some, was in 1815, when it pointed $24^{\circ} 27' 18''$ to the west of north; according to others in 1818, pointing about $24^{\circ} 30'$ west; but the Astronomer Royal makes the maximum at Greenwich $24^{\circ} 23'$, in the year 1819. Since that time it has been gradually diminishing in its westerly declination, and points at present about 20° west of the true north. The inclination of the dipping needle has similarly varied, having been decreasing since the

¹ It can scarcely be considered as affecting this statement that Mr. Burrough, in 1580, mentioned it as being at Limehouse $11^{\circ} 17'$. He does not mention it as a new observation. On the contrary, it may be only another reading of the same. Observations were few at that period, and there is a similar discrepancy in the reading of the dip, $71^{\circ} 50'$ or $71^{\circ} 51'$, though confessedly of one and the same observation.

time when the horizontal needle pointed due north, up to which time it seems to have increased. In 1576 the dip is stated to have been, as observed by Mr. Norman, $71^{\circ} 50'$. There may be some doubt whether the observation was not made even a year or two earlier, some part of Mr. Norman's work, in which it is given, having been written as early as 1561. In a subsequent treatise by Mr. Burrough, published in 1581, it is quoted as "the first and only experiment which hath been made of it²." In 1680

² The date of this observation is important. In the "Philosophical Transactions" for 1673, page 6066, Mr. Norman's observation is, on Mr. Bond's authority, stated to have been made in 1576. Robert Norman, who first observed the variation in London, and who discovered the dip of the needle, states the observed amount of the latter at $71^{\circ} 50'$, in his treatise entitled, "The New Attractive, containing a short discourse of the Magnes or Loadstone," which he dedicated to Mr. Burrough, Comptroller of the Navy, from whom he had received encouragement and advice. At the end of that treatise, in the chapter "Of the Declination of the Sun," he gives "another example *for this year 1561*;" and after working out the problem for the twentieth of April, 1561, there follows the word FINIS, marking the end of Mr. Norman's work. Then follows Mr. Burrough's treatise entitled "A Discourse of the Variation of the Compass or Magnetic Needle, wherein is mathematically shewed the manner of the observation, effects, and application thereof, made by W. B., and is to be annexed to the New Attractive of R.N.," in which he quotes Mr. Norman's treatise, and observation of the dip, in the ninth chapter, calling it "the former treatise by Robert Norman." Now Mr. Burrough's preface to this tract is dated and signed "27th Sept., 1581, William Burrough;" and many of the examples worked in it are of 1580. The two treatises from that time were published together, and the edition which I have seen, "*newly corrected and amended by M. W. B.*," is in black letter, and bears, in its title-page, the year 1596. The observation seems to have been somewhat uncertain between $71^{\circ} 50'$ and $71^{\circ} 51'$; for Mr. Burrough quotes it at the former amount, and immediately treats it at the latter, yet says it was the only observation. Mr. Norman, in his fourth chapter,

the dip appears to have been $75^{\circ} 30'$ at London ; and at Paris, where it has been uniformly of lower amount than at London, it was $75^{\circ} 0'$ in 1671³. I feel justified in assuming that the amount of dip at London when, at the maximum, in 1657 or 1660, was not under $75^{\circ} 45'$, and probably just about that amount. The rate of change in the dip was slower at that point, owing to the curvature of the circle towards us, on a similar account to that which makes the change in the length of the day less rapid at the solstices.

The following table may serve to illustrate the general law ; and may be sufficiently accurate for the purpose, though I find a minute difference in the statement of one or two of the particulars by different writers :—

Variation of the needle at London.			Remarks.
Year.	Declination E or W.	Dip.	
1576	$11^{\circ} 15' E$	$71^{\circ} 50''$	The rate of increase of the dip in the 81 years from 1576 to 1657, as the latter is here assumed, is at an average of $2' 54.074''$ per annum. The rate of decrease of the dip for the 101 years from 1720 to 1821 is at an average of $2' 45.75''$ per annum ; from 1780 to 1821 at the rate of $3' 2.927''$ per annum ; and from 1818 to 1865, $3' 5.1''$.
1620	$6^{\circ} 0' E$		
1657 (Max. Dip.)	0 0	$[75^{\circ} 45' ?]$	
1680		$75^{\circ} 30'$	
1720 ⁴	$14^{\circ} 17' W$	$74^{\circ} 42'$	
1780	$23^{\circ} 17' W$	$72^{\circ} 8'$	At CAPE COMORIN, in India, the needle had attained its maximum declination much earlier, and was returning eastwards, as appears from the following from Dr. Halley. $1620, 14^{\circ} 20' W.$ $1680, 8^{\circ} 48' W.$ $1688, 7^{\circ} 30' W.$
1800		$71^{\circ} 23'$	
1815	$24^{\circ} 27' W$		
1818 (Max. Dec.)	$24^{\circ} 30' W$	$70^{\circ} 34'$	
1821 ⁵		$70^{\circ} 3'$	
1880	$24^{\circ} 2' W$	$69^{\circ} 38'$	
1843 ⁶		$69^{\circ} 1'$	
1850	$22^{\circ} 29' W$	$68^{\circ} 48'$	
1865	$21^{\circ} 6' W$	$68^{\circ} 9'$	
1868 ⁷		$67^{\circ} 56'$	
1870	$19^{\circ} 53'$	$67^{\circ} 52'$	

gives an account of the manner of construction of the instrument with which the observation was made.

. For notes to references 3 to 7, see next page.

(2.) It is sufficiently manifest from these particulars that though the needle in its declination is returning westward, yet, the attractive, or rather directive force which moves it, is not returning on its former

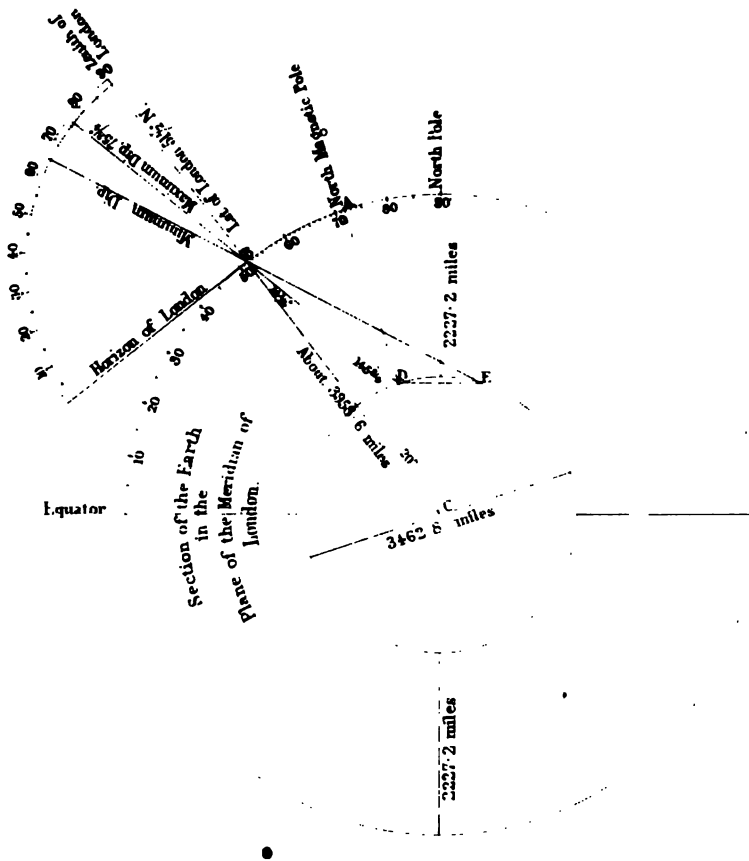
* By an apparent error of the pen or the printer, the dip at London, in 1680, is stated, I believe on the authority of Mr. Bond, at $73^{\circ} 30'$. In Sir David Brewster's article on Magnetism in the "Encycl. Brit.," 7th edition, the dip at Paris, in 1671, is given as above, $75^{\circ} 0'$. The higher amount of the dip at London may be observed in the following instances: in 1780, at Paris, $71^{\circ} 45'$; at London, $72^{\circ} 8'$; in 1791, Paris, $70^{\circ} 52'$; in 1790, London, $71^{\circ} 53'$; in 1818, Paris, $68^{\circ} 35'$; London, $70^{\circ} 34'$.

* The dip in 1720 is given, $74^{\circ} 42'$ by Sir David Brewster, from the observations of Mr. Graham; and, in the popular treatises of Dr. Fergusson on Electricity, and of Mr. Brooke on the Elements of Natural Philosophy, it is so accepted. There may be some slight uncertainty of the exact amount. Mr. Whiston's determination of the dip in 1720 (of which Mr. Cavendish remarked in the "Philosophical Transactions" that he believed it to be pretty nearly correct) was $75^{\circ} 10'$, and General Sir Edward Sabine (then Captain Sabine) accepted it as probably correct ("Phil. Trans." vol. cxii). If so, there is the more reason for not placing the *maximum* dip lower than $75^{\circ} 45'$. But $75^{\circ} 10'$ for the year 1720 would be too far from Mr. Graham's observed amount, which on the whole I prefer to retain, as better agreeing with the general series, as far as I have the means of judging, while, at the same time, resting on the direct evidence of observation unmodified by theory.

* Sir David Brewster, from observations of Colonel Beaufoy, gives the maximum declination in 1815, $24^{\circ} 27' 18''$ W. Mr. Brooke puts it in 1818, and makes the amount $24^{\circ} 30'$, which more nearly accords with observations of Colonel Beaufoy at Bushey Heath, quoted by Mr. Edward Walker ("Terrestrial and Cosmical Magnetism," Camb. 1865, p. 17). But according to the Greenwich Observations, the Astronomer Royal inclines to place it in 1819.

* General Sabine's observation with Meyer's needle, August, 1821, corrected by him by means of other tests. "Phil. Trans." vol. cxii. 19.

* At Greenwich. Sir George B. Airy's "Treatise on Magnetism," pages 23, 94.



In the triangle B C D.

Sine L D. $145\frac{3}{4}$: Sine L B. $14\frac{1}{4}$:: Side B C = 3358.6 miles : CD = 1731.4 miles.

Therefore the diameter of the nucleus is 3462.8 miles.

course, but advancing in a circle round the north pole, and the nearest approach of it to us was in that portion of its circuit when it was between us and the north pole, and when the dip was therefore greatest. When it next comes to the line of no declination, it will be at the remoter part of its circuit, on the other side of the pole, and the dip will then be at the minimum. This will at once explain why at Cape Comorin (as shown in the table) it was already past the most westerly part of its circuit regarded thence, and was passing round on the remoter side of the circle, beyond the pole, thus diminishing the westerly declination at Cape Comorin, while it was still constantly increasing the westerly declination at London, and was far from having reached the maximum. This may be illustrated by a diagram of a section of the earth in the plane of the meridian of London, and, in order to avoid the complicated consideration of other planes, assuming the time when the needle at London pointed due north, and the dip was at the maximum. It will be seen that this furnishes approximately data for calculating the depth down to the pole of the central nucleus, by the triangle BCD .; of which the side BC is the earth's semidiameter, and the angles are given, at C equal to the difference of latitude, 20° ; at B equal to its vertical angle, which is measured on the quadrant from the zenith of London, $14\frac{1}{4}^\circ$; and therefore at D $145\frac{3}{4}^\circ$, being the supplement of their sum to 180° . It is to be remarked that these data will give only an approximate result, because the magnetic ray to the north magnetic pole is thus alone taken account of, whereas the magnetic ray to the south magnetic pole of the earth has also to be considered. At the magnetic equator both these rays act equally upon the dipping

needle, and, balancing one another, produce the effect of *no* dip. As we move northward from that line the north magnetic pole becomes gradually predominant in its influence, but at first greatly qualified by the attractive power of the south magnetic pole; and the dip is the result of the balance of the two. In higher latitudes the predominance of the nearer magnetic pole in regard to the dip becomes more and more decided, and at last practically exclusive. The magnetic latitude of 70° is so high as to reduce the comparative force of the southern magnetic ray to a small proportion; for 70° north magnetic latitude is only 20° from the north magnetic pole, but 160° degrees from the south. Moreover, it is impossible for us to take it into account at the first, for we know not at the outset in what way precisely the proportionate influence of the southern pole is to be measured, whether by the inverse duplicate ratio of the square of the length of the direct way, or modified by the distance measured in degrees of the latitude, or by some other principle dependent on electric currents transverse to the needle^a. It is therefore

^a Measured inversely as the square of the distance in the direct ray, at 70° north magnetic latitude, according to my figures, the power of the southern magnetic pole of the earth upon the magnetic needle would be to that of the northern as 1 to 5.463, or nearly as 1 to $5\frac{1}{2}$. But in regard to its separate effect upon the two poles of the needle, the reciprocal action of the magnetic *needle* upon the earth must be taken into account, and the true result will be the *product* of the two. Now in comparison with the whole magnetic power of the earth, the whole magnetic power of the needle may be regarded as infinitely small; but since action and reaction are equal, the magnetic force of the needle, though exerted towards the earth, will react upon itself, as a man pushing away the land with an oar in effect pushes the boat; and, in the effect upon the needle's own position on its centre, it will be

necessary for us to feel our way to the exact truth in successive steps, by a method of approximation, allowing for correction afterwards. Considering, therefore, at first only the north magnetic ray, as if at our high magnetic latitude it alone were operating, let us obtain the results : and be it borne in mind that when the magnetic pole passed between us and the direct north, the magnetic latitude of London probably amounted to 70° , so that the distance of the opposite magnetic pole was very great, and its effect upon the dip here small.

Basing our calculation, then, upon the data named above, and neglecting for the present the oblateness of the earth's spheroidal figure, but taking it as a sphere of equivalent magnitude, with a diameter of 7917·2 miles, this would give the semi-diameter of the nucleus whose pole revolves = 1731·4 miles ; and the depth of the nucleus beneath the surface of the earth, or, in other words, the thickness of the solid body of the earth down to the nucleus, 2227·2 miles.

The construction of the diagram will probably be

of great amount. Since, then, the magnetic force of the *needle* in relation to the poles of the great magnet of the earth is *also* inversely as the square of the distance, the *product* will be inversely in the *duplicate ratio of the square* ; or as the *fourth power* of the distance respectively from each pole of the magnetic nucleus of the earth.

Therefore, as respects the dip of the needle at any particular place, the effect of the action of the southern pole of the earth's magnetic nucleus, contrary to the northern, may be expected to be inversely in the duplicate ratio of the squares of the distances of the two poles of the nucleus from the *needle* : and, at 70° north magnetic latitude it will thus be not as 1 to $5\frac{1}{2}$, but as 1 to $30\frac{1}{2}$ (Compare Sir G. B. Airy's "Treatise on Magnetism," p. 135). This is but a small proportion ; but it is difficult to suppose that even this, small as it is, will not be still further reduced or modified by electric currents and the earth's rotation.

obvious on inspection. The meeting of the line of maximum dip at London (BD), with the line of vertical dip (AC) at the point D , gives the radius of the nucleus, CD . The cord DE , drawn to the point E , in the circumference of the nucleus, equidistant on the other side of the earth's axis, is the diameter of the circle in which the secular revolution of the magnetic pole is made; and a line drawn from E to the latitude of London, and produced to the graduated quadrant, gives the degree of minimum dip at London, which will be reached at that point of the revolution, subject to such modification as other circumstances may cause. The present dip cannot be correctly expressed in the diagram because it lies in another plane. The line indicating it would have to be conceived as pointing to a place in the circumference of the circle of which DE is the diameter; but that circle in the diagram lies towards us edgewise, and the point to which the line should be drawn would be on the farther edge of the circumference.

(3.) The secular variation of the magnetic needle, then, is caused by a slow and gradual advance of the pole of the diurnal rotation of the *nucleus* (which only retains the old diurnal rotation of the earth itself), making a long circuit round the new pole and axis of the earth's present rotation. The rotation of the nucleus, differing in its equatorial plane from the rotation of the solid body of the earth, and being that of a fluid though heavy substance, with a still heavier solid central part, must give rise to a certain friction upon the solid sides of the interior vault or dome of the earth's solid body within which it moves; and thereby doubtless it generates electromagnetic currents and phenomena, which are modified in their effects, or in their transmission to us

through the strata, by one principal cause which will presently be mentioned, and by various local causes, partly from the presence in some places of magnetizable ores, partly depending on the irregular and unequal interior surface of the dome, or hollow sphere, within which the nucleus revolves, which will needs break and turn the superficial currents of the nucleus. For the nucleus itself is probably fluid for some depth all round, and is, accordingly, subject to tides from the attraction of the sun and moon. But of these two bodies the effect is different, on the same principle as on the tides at the surface of the globe, only with due regard to those particulars in which the cases differ. The effect of the sun's attraction upon the globe of the earth is vastly greater than that of the moon; but his immense distance, in proportion to the earth's diameter, makes the *difference* of his attraction at the nearer and farther side of the earth comparatively little; and, when the diameter is reduced to that of the nucleus, the difference of his attraction at the opposite sides of it must be still less perceptible. But as the nucleus is wholly fluid for some depth, and has its solid centre poised in fluid by its own gravitation and specific weight, the effect of the sun is to draw the whole central mass of the nucleus somewhat towards the nearer side of the earth with an increased pressure there, and thus to produce only one perceptible tide in the day. The sun's effect also is thought to be perceptibly increased annually, in the winter season of the northern hemisphere, the summer of the south, when the earth is in the part of her orbit nearest to him: and this effect takes place in both hemispheres alike and at the same time. The moon, on the other hand, being much weaker in her attractive

power, but being so much nearer to the earth, and her power therefore, such as it is, differing very sensibly at the nearer and the farther side of the nucleus, because of the very sensible proportion which the diameter of the nucleus bears to the moon's distance, and because her effect, at the one side and at the other, is inversely as the *square* of her distance from the one and from the other: the result is, that her effect upon that heaviest portion of the earth is little on its whole mass, but tells upon the circumambient fluid of the nucleus, as upon the seas of the earth's surface, and is perceptible in *two* diurnal tides.

The secular revolution of the pole of the nucleus around the pole or axis of the earth appears to be accomplished in about six hundred and forty years. The exact time cannot yet be stated. This long revolution is analogous to the slow revolution of the pole of the earth round the pole of the ecliptic, which takes place in the recession of the equinoxes, and its cause is also analagous. Sir Isaac Newton pointed out, and subsequent astronomers have confirmed his discovery, that the cause of the recession of the equinoxes is the action of the sun and moon upon the protuberant matter of the earth's equatorial regions, arising from her oblate spheroidal form. Now in regard to the inner globe, or nucleus, while it is acted upon to the production of tides by the sun and moon, the plane of the earth's protuberant equatorial ring is at an angle to the equator of the nucleus, say of $18\frac{1}{2}$ degrees, and must necessarily affect the equatorial plane of the nucleus and its pole by slowly twisting their position. Apart from the action of the sun and moon, that ring would act equally on the whole plane of the equator of the nucleus; but

the sum or result of the actions of the sun and moon on a part of the circumference of the nucleus draws it from that equable effect, and causes it to revolve slowly, yet much faster than the recession of the equinoxes. For though the cause is analogous, the circumstances are very different. The principal motive power in the recession of the equinoxes is the attraction of the moon, slightly turning the position of the plane of the earth's equator in relation to the plane of its orbit. But, in the other case, there is a third body which is introduced, and the principal moving power becomes the earth's protuberant equatorial ring; while the sun and moon would, on the whole, tend to hold the nucleus to its old position. Hence the principal moving power is nearer, the body acted on smaller, and probably more rapid in its rate of rotation⁹; and we need not wonder if, in the result, the secular revolution of the magnetic pole is more rapid than the revolution of the equinoxes in the proportion of about forty-two to one. It is also in the same direction, contrary to the direction of the earth's rotation and revolution in its orbit. For if we compare the movement of the earth's pole, in its revolution of 25,868 years round the pole of the ecliptic, which is necessarily to

* That it may probably have a more rapid rate of rotation than that of the body of the earth appears from its possessing detachment and freedom so to move, combined with the fact that the rate of rotation is, other things being equal, inversely as the square of the radius of the rotating body. With a large allowance for retardation from the conditions and the medium in which the nucleus moved, a considerably greater velocity than the exterior body of the earth would thus be conceivable. Again, the density of the nucleus being much greater than that of the earth generally, from this cause also there would be comparatively more force and tenacity of its own rotation.

the westward, while the equinoctial points are receding on the ecliptic; just so the revolution of the magnetic pole is to the westward¹.

(4.) The magnetic phenomena are in agreement with these principles of the causes from which they result. Mr. Barlow showed that the effects resulting from magnetism might be imitated by currents of galvanic electricity passed through a wire wrapped in parallel coils round a wooden globe, each turn of the wire representing a magnetic parallel, and the two ends of the coil the magnetic poles. When a small magnetic needle was placed on the globe, so prepared, its declination and dip bore a resemblance to those of a needle similarly situated on the earth's surface.

Now that quicksilver is capable, by its motion and friction when shaken in a glass tube, of producing electrical light and phenomena, was long ago remarked²; and it has been also employed as a convenient conductor in some electrical and magnetic experiments, in which rotatory motion has been produced. The nucleus of the earth, revolving with its axis inclined at an angle to the earth's pole, would therefore account for the generating of such currents, and at great depths³, while the previous structure of

¹ "It likewise follows that this motion is westwards."—Dr. Halley, "Phil. Tr." 1692. "The node of the magnetic equator near the island of St. Thomas has moved 4° from east to west between 1825 and 1837."—Humboldt's "Cosmos," vol. i. p. 178. "It is believed that the magnetic poles are rotating round the geographical poles from east to west."—Sir G. B. Airy's "Treatise on Magnetism," p. 53.

² Experiments by M. l'Allamand, "Phil. Tr." 1746, vol. xlv., p. 58.

³ "The source of magnetism must be deep."—Sir G. B. Airy's Treatise, pp. 101—103. "The galvanic currents discoverable at the earth's surface do not explain ordinary terrestrial magnetism. If that

the strata of the earth, in transmitting, might modify those currents. Again, it was the hypothesis of Dr. Halley, that the earth itself was a magnet, and that it had a nucleus also magnetic, the nucleus having two magnetic poles, and the earth two magnetic poles; and that the nucleus revolving slower than the earth about the same axis, its poles receded, relatively to the other. There is an approximation here to the true account of the cause; but Halley supposed the revolution of the nucleus to differ only in being slower about the same axis⁴. In this his conjecture failed of the truth, and was indeed farther from it than Bond's, who, as Halley informs us⁵, supposed two magnetical poles, *and an axis, inclined to the axis of the earth*: only Halley's theory had the advantage of admitting two fixed magnetic poles (or rather directive points) in the earth, besides the two moving ones. But again, while Dr. Halley supposed that the nucleus revolved in fluid, he assumed that fluid to be like water, for he thought all known fluids unfavourable to electricity: by which at the same time the difficulty of a solid nucleus obtaining and keeping its relative position was increased; though that is partly attempted to be answered by him, by a reference to the maintenance of the relative position of Saturn within his ring. It does not appear to me that that analogy obviates the main difficulty; for how was the solid exterior vault formed, within which the nucleus gravitates, if, between the nucleus and the vault, the fluid interposed was so much lighter than the strata above

magnetism is to be explained by such currents, they must be very deep."—Sir George Biddell Airy, *Ibid.* p. 217.

⁴ Halley's second paper, "Phil. Trans." 1692, p. 568.

⁵ First paper, "Phil. Trans." for 1683.

it, that they would necessarily have fallen into it, and never could have floated on it, or been formed and solidified upon its surface. Further, Dr. Halley supposes the existence of some difficulty owing to possible leakage of the water. Nevertheless that hypothesis, formed by that distinguished astronomer, the personal friend of Newton, may satisfy some that there is nothing at variance with science in the supposition of a nucleus revolving somewhat differently from the earth itself⁶. And in support of this

* The theory of Dr. Halley on the subject was printed in two papers in the "Philosophical Transactions," the former in the year 1683, the latter in 1692.

It may perhaps be of some convenience to the reader to specify here some of the many other important papers on the subject of Magnetism in the "Philosophical Transactions," and I may mention the following:—A letter in vol. I., dated Nov. 1757, by William Mountain and James Dodson, concerning the variations of the Magnetic Needle, with tables exhibiting the result of 50,000 observations from 1700 to 1756; a paper in vol. cxii., by Captain, now General, Sir Edward Sabine, 1821; a paper in 1823, vol. cxiii., containing some experiments of Mr. Barlow; and immediately following in the same volume, a paper by Mr. Samuel Hunter Christie, on the Diurnal Variations. In the "Transactions" for 1825 an interesting and important paper by Mr. Barlow, on the effects of the rotation of an iron globe upon the magnet; and in the same volume, a paper by Mr. Christie, on the effects of the revolution of an iron circular plate placed as a tangent to the sphere of which the centre was the centre of the magnet. In the "Transactions" for 1847 and 1850, papers by Colonel Sabine; and in the "Transactions" for 1851, at p. 123, a paper by the same, on the periodical laws discoverable in the mean effects of the larger magnetic disturbances; and at p. 635, on the annual variation of the magnetic declination at different periods of the day; in the same volume, p. 287, Captain Elliott's magnetic survey of the Indian Archipelago; in 1853, a paper by Colonel Sabine; and, connected with this, a paper by the same, in 1856, on the lunar diurnal variation at Toronto; in several successive volumes, particularly cxli. and cxlv., a series of papers by Pro-

I may cite the opinion of a later very distinguished man of science, the late Dr. Robison, professor of Natural Philosophy in the University of Edinburgh, and secretary of the Royal Society there:—"There is nothing repugnant to our knowledge of the universe," says he, "in the supposition of a magnetic nucleus revolving within this earth'." But Halley's

Professor Faraday, since separately published; in the "Transactions" for 1863, a paper by General Sabine, President of the Royal Society, on the results of the magnetic observations at the Kew Observatory from 1857 and 1858 to 1862 inclusive; and immediately following it a paper by Sir G. B. Airy, Astronomer Royal, on the diurnal inequalities of terrestrial magnetism, as deduced from observations at the Royal Observatory, Greenwich, from 1841 to 1847; and, in vol. clix., in the year 1869, a continuation of the subject by Sir G. B. Airy, on the diurnal and annual variations, from observations at Greenwich, from 1858 to 1863; with a note on the luno-diurnal and other lunar inequalities, from observations extending from 1848 to 1863. To these must be added a very important series of contributions to terrestrial magnetism by General Sir Edward Sabine, copiously embodying the results of the great national undertaking of the magnetic survey of the south polar regions of the globe, 1840—1845. The observations of the first year's survey, with their results, are in the "Philosophical Transactions" of 1843; those of the second year in the "Transactions" of 1844; and those of the third in the "Transactions" of 1866; these forming respectively the fifth, sixth, and tenth numbers of those "contributions." The results of the survey of the additional ship "Pagoda" are in the eighth number; and in the "Transactions" of 1868 is number eleven, containing the completion of the review of the survey, with maps of the results.

' Article on "Variation," published in the third edition of the "Encyl. Brit.," and article on "Magnetism" in the supplement of that edition. In the latter of these Professor Robison qualifies his statement thus:—"The hypothesis of Dr. Halley that the globe which we inhabit is hollow, and encloses a magnetic nucleus, moving round another axis [this was not Halley's view], is not inconsistent with any natural law, if he did not suppose the interval filled up with some fluid. The action of the nucleus and shell on the intervening fluid would gradually bring the two to one

magnetic theory has sometimes been unworthily treated, by confounding it with some speculations of his, which, though lightly put forth in the same paper, are no part of his magnetic theory, but are put by him expressly only to parry the speculative objections of others as to there being no conceivable use for such an interior structure of the world; and hence they may rather be traced as referring to Dr. Thomas Burnett's theory of the earth, then lately published. There is even a spice of satire in what he says:—"Whereas I have ventured to make these subterranean orbs capable of being inhabited, 'twas done designedly for those who will be apt to ask *cui bono*, and with whom arguments from final causes prevail much. I am sure the poets, Virgil and Claudian, have gone before me in this thought; . . . and though this be not to be esteemed an argument, yet I may take the liberty I see others do, to quote the poets when it serves my purpose." Thus he meets his opponents with their own weapons; but these are no part of his magnetic theory. Halley was not a man whom even Baron Humboldt was entitled to lampoon^s, or whose theory of magnetism Sir David

common motion of rotation, as may be inferred from the reasonings employed by Newton in his remarks on the Cartesian vortices." The subsequent progress of science seems to me to have removed or much qualified this difficulty of Dr. Robison's, by the office which quicksilver has been made to discharge in the experiments of electro-magnetic rotatory movement, and by the strong adherence of a massive and heavy rotating body to its plane and axis of rotation. The gyroscope illustrates this in the case of a very oblate figure revolving round a fixed centre. In a sphere or slightly oblate spheroid, it is of course by no means equal, but still it is such that, without some change in the balance of its weight and position of its centre of gravity, it would not readily be overcome.

^s "Cosmos," i. 163.

Brewster should have presumed to call "absurd," especially, since, after Gilbert, magnetic science owes nearly its first firm groundwork of induction to Halley. It appears to me, that the first suggestion of his theory was probably owing to Mr. Burrough's calculation of the depth of the north magnetic "point respective" (as he appropriately terms it), which, by an application of spherical trigonometry, reasoning from the intersection of two magnetic meridians, and from the dip of the magnetic needle, then newly discovered, he in 1580 concluded to be "from the centre of the earth 1085 miles (after that rate of 60 to a degree at the equator, and $3436\frac{4}{11}$ for the semi-diameter of the earth); and the distance of the same from the axis of the world, 471 miles' : " a measurement which, when reduced from nautical miles to the common reckoning of about $69\frac{1}{2}$ to a degree at the equator, will be found not so very remote, as regards the radius of the circle of secular revolution from that which by other processes, and while in ignorance of his calculation, I arrived at. And the idea of a nucleus revolving somewhat differently from the earth may be more favourably considered, when a cause is at length shown, in accordance with

* Article "Magnetism," "Encycl. Brit.," 7th edit.

¹ Chapter 8, of Mr. Burrough's Discourse, appended to Robert Norman's "New Attractive." These measurements are equivalent to 545·58 miles for the radius of the circle in which the magnetic pole of the nucleus performs its secular revolution, and 1256·8 for the semi-diameter of the nucleus. The former is within about four miles of that which I have found; but its proportion to the relative size of the nucleus deducible from his figures is much greater, and the semi-diameter of the latter is much less, than I have calculated. The consequence would therefore be a much greater angle of inclination of the axis of the nucleus to that of the earth, than that which the facts appear to me to prove.

which that difference between the revolution of the earth and of the internal nucleus is accounted for in harmony with all the facts.

(5.) By an important scientific application of photography to the automatic registration of the variations of the magnetic needle, for the invention of which we are indebted to Charles Brooke, Esq., F.R.S.², and which is now in constant use in the observatories at Greenwich, Kew, Paris, and the observatories established by the British Government at Toronto, Hobarton, St. Helena, and the Cape of Good Hope, a more exact continuous and complete knowledge of the minutest variations, by night and by day, has been obtained. General Sir Edward Sabine and the Astronomer Royal have each published in the "Philosophical Transactions," the results of their careful comparison, and the investigation of long series of these variations. Some of them are found to depend on the sun, some on the moon. The observed effects of the lunar action and of the solar action are widely different. The lunar action is semi-diurnal; the solar action is mainly diurnal, partly also annual. The direction of the solar annual inequality is the same in the northern and southern hemispheres; the north end of the magnet pointing more to the east, both at Kew and at Hobarton, when the sun is north of the equator, and to the west when the sun is south of the equator. From this it plainly appears to be determined not by the summer or winter temperature, or the latitude, but by the earth's perihelion; that is, by the increased force of the sun upon the earth's central part (irrespectively of the position of the place of observation

² "Phil. Trans." 1847.

north or south), at the portion of the earth's orbit at which the earth comes nearest to the sun. The solar diurnal variation is that which is repeated every succeeding solar day; the north pole of the magnet moving towards the west from sunrise to about an hour after noon, when it begins to retrograde, and continues moving towards the east till eight in the evening, after which it remains nearly stationary till next sunrise, thus exhibiting one tide in the twenty-four hours.

To separate and ascertain the amount of diurnal change due to lunar influence, General Sabine commenced by marking every observation in small figures on the face of the monthly tables, with the lunar hour to which each observation most nearly corresponded. A fresh set of monthly tables was then prepared for each month in each of the five years included in his investigation; in which tables there were entered the differences (each under the lunar hour to which it most nearly corresponded,) between the several observations, and their corresponding normals. By this proceeding the diurnal and other variations, depending on the time of the solar year, and on the hour of the solar day, were in great part at least eliminated. The total number of observations examined and compared in the investigation was, of the Bifilar or horizontal magnet 34,303, and of the Vertical Force magnetometer 31,773.

Thus proceeding by careful and judicious analysis of the observed facts, General Sir Edward Sabine succeeded in establishing the reality of a lunar influence, of minute amount, but of extreme regularity. The general conclusion to which he was conducted presented a remarkably symmetrical double progression in each lunar day. "The de-

clination," he observes, "has two easterly and two westerly maxima in the interval between two successive passages of the moon over the astronomical meridian, and the Inclination and the total Force have each two maxima and two minima due to the moon's action in the same interval; the variation passing in every case four times through zero in the lunar day. The easterly maxima of the horizontal Deflection of the north end of the magnet synchronize with the moon's superior and inferior passages of the meridian; the westerly maxima with the *lunar* hours of 6 and 18. The maxima of the increased magnetic Force due to the moon's action occur about the *lunar* hours of 3 and 16, and the minima about the hours of 9 and 20. The maxima of the Inclination, that is, of the dip of the north end of the magnet, occur about the lunar hours of 3 and 14, and the minima about 9 and 20. The extent of the variation in the lunar day, or the range between the extremes that are widest apart, is, in the Declination, $38^{\circ}33'$, in the Inclination $4^{\circ}4'$, and in the total Force $\cdot 000012$ parts of the whole magnetic Force at Toronto." These measures are calculated from the Toronto observations. He adds that, "We have a better opportunity of judging of the particular nature and character of the moon's magnetic influence by studying the effects produced on the declination than on either of the other elements³." Of this semi-diurnal lunar influence on the earth's magnetism, it has been justly remarked that it closely resembles the great tide-wave of the ocean. Its peculiar character is quite acknowledged. Speaking of this lunar influence on the magnetic movement, the Astronomer Royal says,

³ "On the lunar diurnal variation at Toronto." "Phil. Trans." 1856, p. 499.

“The inequalities are so evidently semi-diurnal, that we may at once proceed to treat them on that assumption.” Even in the magnetic *disturbances* which occur more irregularly there is found to be some great deep-lying law connecting remote parts of the globe. At Nertschinsk, in Siberia, (long. $117^{\circ} 34' E.$, lat. $51^{\circ} 52'$) the curve of westerly deflections, as Mr. Brooke states, corresponds very closely with that of easterly deflections at Kew and Hobarton, but the minimum occurs just twelve hours later, and the maximum in like manner. General Sabine has thought that both an annual and a diurnal law may be traced in the larger magnetic disturbances.

(6.) There is also an observed and very striking connexion in many cases between these disturbances and the occurrence of the aurora borealis; not that the aurora causes the disturbance, but both probably being results of the same causes. The sagacious mind of Dr. Halley had discerned this relation long ago, and it has since been more fully proved, as Baron Humboldt admits. “The bold conjecture,” says he, “hazarded 128 years since by Halley (*Phil. Trans.*,” vol. xxix. for 1714-1716, No. 341), that the aurora borealis was a magnetic phenomenon, has acquired empirical certainty from Faraday’s brilliant discovery of the evolution of light by magnetic forces⁴.” On the other hand, lightning and atmospheric electricity do not affect the magnet, except where it is placed in conjunction with the wires of the telegraph. But Baron Humboldt has remarked that the system of the clouds is very perceptibly affected by magnetism; and the fact is so important that I will subjoin the passage.

⁴ “Cosmos,” vol. i. p. 187.

“This connexion of the polar light with the most delicate cirrous clouds deserves special attention, because it shows that the electro-magnetic evolution of light is a part of a meteorological process. Terrestrial magnetism here manifests its influence on the atmosphere, and on the condensation of aqueous vapour. The fleecy clouds seen in Iceland by Thienemann, and which he considered to be the northern light, have been seen in recent times by Franklin and Richardson near the American North Pole, and Admiral Wrangle on the Siberian coast of the Polar sea. All remarked ‘that the aurora flashed forth in the most vivid beams when masses of cirrous strata were hovering in the upper regions of the air, and when these were so thin that their presence could only be recognized by the formation of a halo round the moon.’ These clouds sometimes range themselves, even by day, in a similar manner to the beams of the aurora, and then disturb the course of the magnetic needle in the same manner as the latter. On the morning after every distinct nocturnal aurora, the same superimposed strata of clouds have still been observed that had previously been luminous. The apparently converging polar zones (streaks of clouds in the direction of the magnetic meridian), which constantly occupied my attention during my journeys on the elevated plateaux of Mexico, and in northern Asia, belong probably to the same group of diurnal phenomena.”

“On my return from my American travels,” he adds in a note, “I described the delicate cirrocumulous cloud, which appears uniformly divided as if by the action of repulsive forces, under the name of polar bands (*bandes polaires*), because their perspective point of convergence is mostly at first in

the magnetic pole, so that the parallel rows of fleecy clouds follow the magnetic meridian. One peculiarity of this mysterious phenomenon is the oscillation, or occasionally the gradually progressive motion of the point of convergence. It is usually observed that the bands are only fully developed in one region of the heavens, and they are seen to move, first from south to north, and then gradually from east to west. I could not trace any connexion between the advancing motion of the bands and alterations of the currents of air in the higher regions of the atmosphere. They occur when the air is extremely calm, and the heavens quite serene, and are much more common under the tropics than in the temperate and frigid zones. I have seen this phenomenon on the Andes, almost under the equator, at an elevation of 15,920 feet, and in Northern Asia, in the plains of Krasnojarski, south of Buchtarminsk, so similarly developed, that we must regard the influences producing it as very widely distributed, and as depending on general natural forces⁵."

(7.) It may not be irrelevant to mention, that I have myself had opportunities of observing a very close connexion between the larger sort of solar and lunar halos or coronas and the magnetic phenomenon of the aurora borealis. In August and September of the year 1870, there were repeated displays of the aurora borealis in Scotland, which were remarked at the time. On the 20th of August, and on the 3rd of September, I observed it well developed. On the 14th September, 1870, about a quarter before twelve at night, at West Kilbride, Ayrshire, I saw and pointed out to several of my family a remark-

⁵ "Cosmos," vol. i. p. 191, 192.

able halo round the moon at a great distance from it, consisting of white filmy clouds, very thin and light, which had arranged themselves in an exact circle, the whole interior of which was cloudless and of a darker sky. The halo was without any tinge of colour, and the moon bright and clear upon the sky in the midst. Streaks of filmy white cloud, of the nature of the aurora borealis, extended from the margin of the halo across to the western and north-western portion of the sky, indicating their nature by the peculiar waxing and waning of their intensity. The night was calm, and next morning the same long streaks of white filmy cloud were still visible in nearly the same position, but of course without the circle of the halo. About two o'clock of that day, a remarkable halo round the sun, of exactly similar nature and dimensions, was seen by me, and pointed out to two of my sons and to another person who was with us on the shore. The whole interior of the circle of the halo was cloudless, and of a darker tone of sky than that exterior to the circle. Again, two days after, a little after five o'clock p.m., walking with one of my sons, I observed a similar halo round the sun, which was shining at that time through a thin portion of some calm unmoving clouds; and on that last occasion there were long filmy streaks of thin white cloud stretching upward and outward from the circle of the halo, in a direction diverging from the sun. These, when attentively observed, waxed and waned in intensity, as does the aurora borealis. I plainly perceived that the phenomenon before me was the aurora, only seen by daylight, and that the upper segment of the halo, with its dark included sky, if seen when the sun is considerably below the horizon,

would become the auroral segment or arch of dark cloud in the northern or north-western horizon, from which, in very numerous cases, the northern shoots of filmy auroral light are seen to rise and to diverge. Thus a connexion of these phenomena with magnetism angularly modifying the influence of the sun and moon upon the atmosphere, appears indicated. The radius of the halos seemed to be from about $18\frac{1}{2}$ to about 20 degrees, or more.

I have seen only two other solar halos of the same description, one slightly tinged with colour. But in all the instances I particularly remarked the darker hue of the sky enclosed within the circle. In this respect they are precisely the converse of the rainbow, in which, when bright and perfect, the interior sky and landscape is much lighter than the exterior; so that I have seen it like a glorious triumphal arch in a dark lofty wall, through which a bright landscape was beheld. It appears indeed not to have been generally noticed that the coloured arch of the rainbow is thus the fringe of a great disk, which is, as it were, the immensely magnified, and proportionably faint, reflection of the solar disk upon the cloud and landscape. These circumstances seem to bring the rainbow into a direct relation to the same magnetic class of phenomena. The general magnitude of it gives the same indication, being of just about double the diameter of the solar halos, which arises, I think, from its being represented on the opposite concave of the sky, the spectator being midway; so that the prolonging of the cone of light which has the sun for its apex, gives the magnitude in the opposite concave double the size. The prismatic analysis of the rays of light, which has resulted of late in so many important consequences,

does not interfere with these facts, nor alter the relation of these several phenomena to one another. The principles on which the prismatic colours are presented on the spray of a waterfall, or on the drops of falling rain, have been fully investigated; but the general uniformity of the size of the rainbow, and its relation to the facts which have been alluded to, distinguish it from other iridescent phenomena: nor do we behold a rainbow on every sunny shower.

(8.) The degree in which the atmosphere is affected by magnetism has been made the subject of some scientific observations. Professor Faraday, in his "Experimental Researches on Electricity," published in the "Philosophical Transactions," demonstrated that oxygen, a constituent part of the atmosphere, is highly susceptible of magnetic influence; and in the twenty-sixth series of these "Researches" (vol. cxli. of the "Transactions") he devotes a section to atmospheric magnetism. "Hence," says he (No. 2863), founding his deduction on the highly paramagnetic quality of oxygen, "the atmosphere is, in common phrase, a highly magnetic medium. The air that stands upon every square foot of surface on the earth is equivalent, in magnetic force, to 8,160 lbs. of crystallized protosulphate of iron." Then in considering how the varying condition of the atmosphere may affect its magnetism, and thence affect the magnetic needle, he has the following remarks at page 53, Nos. 2882 and 2883:—

"Assuming that the axis of rotation of the earth was perpendicular to the plane of its orbit round the sun, and dismissing for the present other causes of magnetic variation than those due to the atmosphere, the two hemispheres of the earth, and the portions

of air covering them, would be affected and warmed alike by the sun, or at least would come into a constant relative state, dependent upon the arrangement of land and water; and the lines of magnetic force having taken up their position under the influence of the great dominant causes, whatever they might be, would not be altered by any annual change due to the atmosphere, since the daily mean of the atmospheric effect in a given place would at all parts of the year be alike. Under such circumstances the intensity and direction of the magnetic forces might be considered constant [*i.e.* as far as depends on the atmosphere], presuming no sensible change to take place by the difference in distance from the sun which would occur in different parts of the world; and, as regards the two magnetic hemispheres, each would be the equivalent of and equal to the other, and they may for the time be considered in their mean and normal state. But as the axis of the earth's rotation is inclined $23^{\circ} 28'$ to the plane of the ecliptic, the two hemispheres will become alternately warmer and colder than each other, and then a variation in the magnetic condition may arise. The air of the cooled hemisphere will conduct magnetic influence more freely than if in the mean state, and the lines of force passing through it will increase in amount, whilst, in the other hemisphere, the warmed air will conduct with less readiness than before, and the intensity will diminish."

I quote these thoughts of Professor Faraday in relation to atmospheric magnetism, simply as showing how highly, in his opinion, the atmosphere is influenced by magnetism, but not at all as showing that terrestrial magnetism, in its main origin, can possibly be referred to any atmospheric cause, or to

any action of the sun or moon upon the atmosphere, or even upon the earth. The reasoning of the Astronomer Royal against the supposition that it is produced in any important degree by magnetic forces external to the earth, or at the surface of the earth, is brief, but entirely conclusive⁶.

The existence of some connexion between magnetism and temperature is thought to be shown by the changes of the variation at St. Helena⁷, according as the sun is north or south, and by the correspondence, remarked by Baron Humboldt, between the isodynamic and the isothermal lines upon the globe; but whether the heat or the magnetism stands more generally in the relation of cause, or whether both are concomitant effects of deeper causes, and reciprocally act and react upon one another, it is unnecessary at present to inquire.

(9.) It is demonstrable from these facts and scientific considerations, that a change of the earth's axis must have greatly and permanently affected the magnetic condition of the atmosphere, the elasticity and condensation of the vapours of the sky, and the whole magnificent system and scenery of the clouds. It is equally clear that these atmospheric changes do not in the slightest degree account for the solar annual variation of the needle, which is the same at the same time in the northern and southern hemisphere, notwithstanding their reversed circumstances of temperature, nor for the lunar tides of magnetism, nor for the great secular changes of declination and dip, but that a common cause must be sought for them all,

⁶ "Treatise on Magnetism," section vi.

⁷ General Sabine "On the Diurnal Variation of the Magnetic Declination at St. Helena," "Phil. Trans." 1847, p. 51; and Professor Faraday, "Phil. Trans." vol. cxli. pp. 96—116.

and that that cause lies deep in the constitution of the earth and its relation to the solar system.

All the facts meet and have their explanation in a change of the earth's axis, such as is specified in this proposition; the axis of the nucleus remaining as it was before, but with two important exceptions, (1) as being carried along with the earth in its new diurnal rotation, and (2) as being affected by the secular revolution of its pole round the pole of the earth's axis, of which the cause and principle have been indicated. The former of these motions, while it is the necessary consequence of the permanency of its relation to the earth within which it moves, involves a peculiar diurnal revolution of each of its poles in relation to the sun and sidereal heavens; its axis remaining coincident at its centre with the centre of the earth, but describing with its two ends, if produced in space, a double cone, somewhat like an hour-glass, with a radius of $18\frac{1}{2}$ degrees for its terminal circles. It happens as a boy sways a spinning top hither and thither in his hand without affecting its rotation.

(10.) There is a complication of the magnetic system in Eastern Asia, in the existence of an elliptic space which forms an exceptional region in the eastern magnetic hemisphere, and which is indicated on the magnetic charts; the isogonic lines (as those are called which trace the equal declination) curving towards a centre of their own, somewhere about N. latitude 60° , longitude 120° East*. An effect of this

* In regard to this, Baron Humboldt remarks, "Two admirable observers, Hansteen and Adolphus Erman, have made us acquainted with the remarkable double curvature of the lines of declination in the vast region of Northern Asia, these being concave towards the pole, between Obdorsk, on the Oby, and Turnchansk, and convex

kind might arise from a considerable portion of the earth's solid interior crust or dome being forced up or broken away, forming a great inverted basin in which the fluid metal of the nucleus perpetually gyrates as in a great whirlpool, instead of being permitted to follow the general current of rotation. This would generate electric currents of that fixed local nature. Such a partial breaking or forcing up of the interior dome was not unlikely to occur in the change of axis; there is a similar case in the South Sea, just at the opposite side of the world, not far from the region where the great continent fell in; and perhaps there may be minor local instances. The rotatory force and friction of the heavy internal fluid upon the interior of a dome which had been adapted originally to a different position of the oblateness, and a different direction of the movement, would be sufficient to account for the formation of such chasms and inverted whirlpools. In part the effects may be modified at the surface of the earth by the cold of that region of Northern Asia, but this I apprehend to be a minor consideration.

between the Lake of Baikal and the Gulf of Ochotsk. In this portion of the earth in Northern Asia between the mountains of Werchojansk, Jakutsk, and the Northern Korea, the isogonic lines form a remarkable closed system. This oval configuration recurs regularly, and over a great extent of the South Sea, almost as far as the meridian of Pitcairn [long. $133^{\circ} 10'$ W.] and the group of the Marquesas Islands [about 140° W.], between 20° north and 45° south latitude. One would almost be inclined to regard this singular configuration of closed, almost concentric, lines of declination as the effect of a local character of that portion of the globe; but if, in the course of centuries, these apparently isolated systems should also advance, we must suppose, as in the case of all great natural forces, that the phenomenon arises from some general cause."—"Cosmos," vol. i. p. 175.

Apart from those two main instances in which I have suggested the probability of the interior of the dome having been broken, it must further be taken into account that the interior surface of the hollow sphere, within which the nucleus revolves, could not be supposed to be perfectly equal and smooth, but rather in a lesser degree to resemble the varied and unequal surface of the earth; so that Mr. Wyld's great globe, long an object of general interest in the metropolis, in which, for convenience, the continents, mountains, promontories, and gulfs, were modelled and represented on the concave, instead of the convex, of the sphere, might suggest, though on an exaggerated scale, some idea of the sort of configuration of the concave within which the interior metallic fluid, revolving with the nucleus, would surge and eddy, like the waters in the hidden caverns, and past the deep projecting rocks and ledges, of the ocean's bed; so as, in the converse position, upward and outward, to present a varied and locally modified current, beneath which the main sweep of the revolution would proceed at greater depths undisturbedly. But these superficial modifications of the current, being exactly where the friction of the interior metallic fluid with the enclosing dome occurred, must necessarily give rise to local modifications, in the development of the electric and magnetic results. And it would be as impossible to reduce these detailed modifications to calculation from one general theory, as it would be to compute the local modifications of the tides in all the seas and estuaries without knowledge of the geography, or local observation of the facts. Nevertheless these modifications do not invalidate the general theory in either case; nor, even when added to the bending or refraction of the electro-magnetic

currents in the course of their transmission upward through the vast intervening mass of strata for two thousand miles, do they prevent the recognition of some one common cause and general law.

(11.) With regard to the position of the southern magnetic pole, which is not indicated by the phenomena to be exactly opposite to the northern, there is a cause which will presently be adverted to. Meantime it should be observed that in the transmission of the magnetic effects to the surface of the earth their nature and observed place may be much modified by the nature of the strata of land and water through which they are transmitted. The refraction of light by water may remotely illustrate this, objects not appearing where they are. The different rate of the transmission of electric currents in different mediums even along the telegraphic wires has been lately remarked. "Professor Gould has found that the velocity of the electric waves through the Atlantic cables is from 7000 to 8000 miles per second, and depends somewhat upon whether the circuit is formed by the two cables or by one cable and the earth. Telegraphic wires upon poles in the air conduct the electric waves with a velocity a little more than double this; and it is remarked as a curious fact that the rapidity of the transmission increases with the distance between the wire and the earth, or the height of the support. Wires buried in the earth likewise transmit slowly, like submarine cables. Wires upon poles but slightly elevated transmit signals with a velocity of 12,000 miles per second, while those at a considerable height give a velocity of 16,000 or 20,000*." If such is the effect even in

* "Journal of the Society of Arts," vol xviii. p. 682. No. for June, 1870.

insulated conducting wires, how much more may differences of vastly greater amount be expected in the transmission of currents where there is no insulation? These particulars may partly suggest the existence of some minor and subordinate reasons for the want of complete correspondence and symmetry between the apparent position of the northern and southern magnetic poles. The same considerations must be kept in view as locally and partially modifying the main causes which will be adduced to account for the difference between the calculated positions of the magnetic pole and the position of it as observed. But the principal reasons of some of the chief anomalies we shall come to by and by.

(12.) Mr. Burrough, in the tract which has already been quoted, calculating from the intersection of two magnetical meridians, would have placed the magnetic pole $25^{\circ} 44'$ from the Arctic pole, in a position which he confesses that his own subsequent experience in his travels contradicted; but he mentions that "Gerardus Mercator placed the magnetic pole $16^{\circ} 22'$ from the pole articke," that is, in north latitude $73^{\circ} 38'$. This is remarkable, since, at a much later period, Gauss's theory gave the two magnetic poles respectively in north latitude $73^{\circ} 35'$, west longitude $95^{\circ} 39'$; and in south latitude $72^{\circ} 35'$, long. $152^{\circ} 30' E.$ ¹. In Halley's day the magnetic pole was estimated by him to be in the meridian of Land's End; and he thought it to be not more than seven degrees

¹ Sir James Ross's "Antarctic Voyage in 1839—1843," vol. ii. p. 446 (Appendix). Gauss had modified this theoretical calculation of the south magnetic pole from data at Hobart Town to lat. $66^{\circ} S.$ and $146^{\circ} E.$, which Sir J. C. Ross found to be quite wrong.

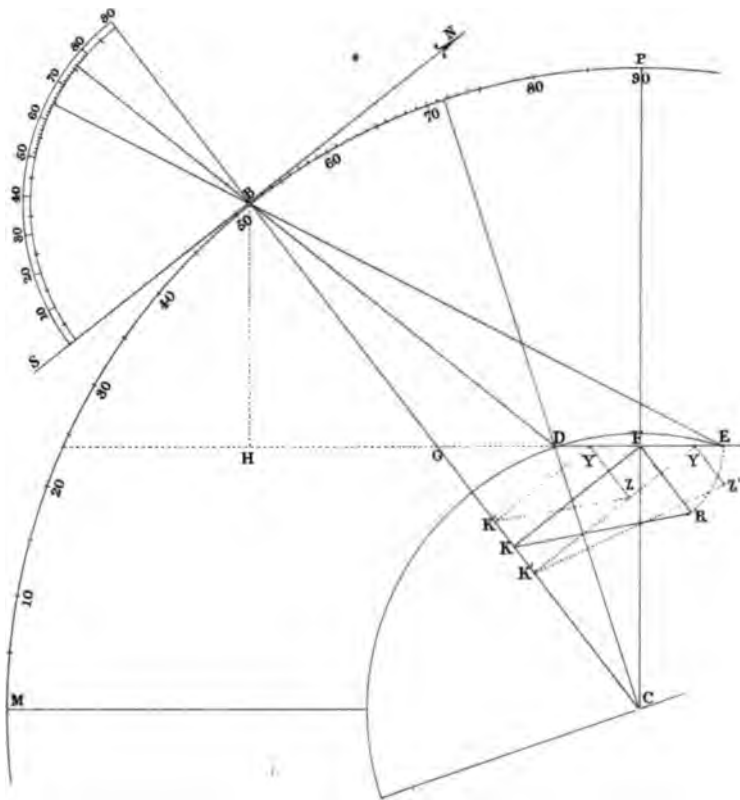
from the pole Arctic : but in that computation he was probably misled by want of acquaintance with a second cause diminishing the angle of observed declination, and so throwing the assumed position of the "point directive" farther north. That cause we shall consider afterwards. Mr. Barlow, in describing his celebrated magnetic experiment with the wooden globe, inlaid with parallel coils of conducting wire, and placed in connexion with a galvanic current, says, "In order to render the experiment more strongly representative of the actual state of the earth, the globe, in the state described, was covered with the gores of a common globe, which were laid on so as to bring the poles of the wire arrangement into the situation of the earth's magnetic poles, according to the best observations we have for this determination. *I therefore placed them (says he) in latitude 72° north, and 72° south, and on the meridian corresponding to longitude 76° west.*" Professor Hansteen, on the other hand, at first placed the principal north magnetic pole in latitude 69° 39' N., long. 87° 19' west from Greenwich, and the principal south magnetic pole in latitude 68° 44' S., long. 131° 47' E., positions which afterwards, on obtaining access to the valuable series of observations made during the British Arctic voyages of discovery, he modified. For he found from Captain Parry's observations, that in 1819 the principal north magnetic pole must have been in north latitude 71° 27' This is in very close agreement with the latitude which has in the present work been assigned to it in correspondence with the main train of the reasoning ; namely, $18\frac{1}{2}^{\circ}$ from the pole of the earth's diurnal rotation. Sir James Ross, however, on the 1st of June, 1831, found the north magnetic pole in latitude 70° 5' 17", long. 96° 46' 45"

W., or came so near it there that the dip was $89^{\circ} 59'$, only one minute from the vertical position. These things are as might be expected from the nature of the causes; and we shall presently indicate a cause which would tend to make the needle vertical, or all but vertical, a little to the south of the true position of the pole of the magnetic nucleus.

(13.) But I must now point out a grand special *inflection* and *deflection* of the needle towards the axis of the earth's rotation, and towards the centre of the whole earth and nucleus, sufficiently accounted for on a principle discovered by Professor Barlow, in investigating the effects of the rotation of a sphere, or mass of iron, on the magnetic needle. This principle, which has been experimentally proved, is also demonstrably operating, and is necessary to reconcile some results otherwise discordant. But first we must indicate the facts and results to be reconciled. If a terrestrial globe be rectified for the latitude of London ($51\frac{1}{2}^{\circ}$ N.), and London be brought to the zenith, and the quadrant of altitude be fixed over it, let the graduated edge of the quadrant be conducted past the observed place of the magnetic pole in 1831 in Boothia Felix (lat. $70^{\circ} 5' 17''$ N., long. $96^{\circ} 46' 45''$ W.), the graduated edge will cut the horizon about $28\frac{1}{4}^{\circ}$ west of north, which greatly exceeds the declination indicated by the needle at that date. In correspondence with this I find on calculation, also, that the assumed position of the magnetic pole of the nucleus, and circle of its secular revolution, as illustrated in the diagram already submitted, would give geometrically a considerably greater angle than the actual one for the maximum horizontal variation of the needle at London, if that

pole were the only cause operating; while, on the other hand, to reduce the conditions to those which would yield the observed maximum of horizontal variation, would reduce the range of variation of the *dip* below what is possible according to the facts. To make this plain, let us adopt the same construction of diagram as before, on a somewhat enlarged scale, to which I must make the following addition :—

In the line DE (which is the diameter of the circle of revolution of the pole of the nucleus round the pole and axis of the earth), from the centre F , draw the line FK , perpendicular to the zenith line of London BC , and from the centre F draw FR , a radius of the circle of which DE is the diameter, perpendicular to FK (FR should strictly be conceived as perpendicular to the plane of the paper; but the angle FKR will be of the same amount, whether it be placed so, or laid down on its side, as in the diagram). Now, if FR be raised and placed perpendicular to the plane of the paper, the reader will perceive at once that FKR is the angle formed between the plane of the meridian of London (which is here the plane of the paper) and the plane of a great circle of the sphere having its diameter in coincidence with the London zenith line BC , as the common line of intersection of the planes, and passing through the point R , at the extremity of the transverse diameter to DE , marking a quadrant of the revolution of the magnetic pole. That, therefore, is the angle which the horizontal variation of the compass at London should be expected to show, at the quadrature of the revolution of the magnetic pole, as far as depends upon these data. Of course we know that the continuity of the planes is practically intercepted by the body



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of the nucleus, but that circumstance may be left out of view for the present, in merely estimating their angular inclination to one another.

Now we have here materials for the determination of that angle. For, first, in the triangle DCF , DC has already been determined (page 113, *diagram*), equal to 1731·4 miles, and DF and FC are found by the following proportions:—

$$(1.) \text{Rad.} : \text{Sine } \angle DCF = 18\frac{1}{2}^\circ :: DC : DF = 549\cdot39 \text{ miles.}$$

$$(2.) \text{Rad.} : \text{Cosine } 18\frac{1}{2}^\circ :: DC : CF = 1641\cdot9 \text{ miles.}$$

Next, in the triangle CFK ,

$$(3.) \text{Rad.} : \text{Sine } \angle FCK = 38\frac{1}{2}^\circ :: CF : FK = 1022\cdot125 \text{ miles.}$$

Again, in the triangle KFR , FR is equal to DF = 549·38 miles, as above.

$$\text{And } (4.) FK : FR :: \text{Rad.} : \text{Tan. } \angle FKR = 28^\circ 15' 27''.^2$$

² I subjoin the logarithmic working of these in their order:—

$$1. \quad \text{Log. Sine } 18\frac{1}{2}^\circ = 9\cdot501476$$

$$DC = \text{log. of } 1731\cdot4 = 3\cdot238397$$

$$\begin{array}{r} 12\cdot739873 \\ \text{Rad.} = 10\cdot000000 \end{array}$$

$$\text{Log of } DF = 2\cdot739873$$

$$2. \quad \text{Log. Cosine } 18\frac{1}{2}^\circ = 9\cdot976957$$

$$\text{Log. of } DC = 3\cdot238397$$

$$\begin{array}{r} 13\cdot215354 \\ \text{Rad.} = 10\cdot000000 \end{array}$$

$$\text{Log. of } CF = 3\cdot215354$$

$$3. \quad \text{Log. Sine } 38\frac{1}{2}^\circ = 9\cdot794150$$

$$\text{Log. of } CF = 3\cdot215354$$

$$\begin{array}{r} 13\cdot009504 \\ \text{Rad.} = 10\cdot000000 \end{array}$$

$$\text{Log. of } FK = 3\cdot009504$$

[4 Log.

To obtain a more general determination of the angle of horizontal Declination for any other position of the pole of the nucleus in its secular revolution, produce DE' , the diameter of its circle of secular revolution, to the circumference of the earth in the plane of the meridian of London, and let it cut the line BC (the zenith-line of London) in the point G ; and from B the latitude of London, let fall upon it the perpendicular BH . This construction we shall have occasion to refer to for another purpose also besides the present. Let now the sine and cosine of any given arc in the circle of secular revolution of the pole of the nucleus be taken. Let the cosine, measured from F , the centre of the circle, be FY , and let the sine be represented by ZY , Z being the given point in the circumference of the circle of secular revolution (which circle must be conceived inserted at right angles to the plane of the meridian and of the paper, as before in the case of FR). From the point Y , draw YK' perpendicular to the zenith-line of London, BC ; place ZY at right angles to YK' ; and join $K'Z$. The angle $YK'Z$ would be the angle of the Declination of the needle towards the pole of the nucleus when at the point Z in the circle of its secular revolution, if the Declination were dependent exclusively on the directive force towards the pole of the nucleus.

This will be perhaps more easily perceived if the

$$\begin{aligned} 4. \text{ Log. of } FR(=DF) &= 2.739873 \\ \text{Rad.} &= 10.000000 \end{aligned}$$

$$\begin{array}{r} 12.739873 \\ \text{Log. of } FK = 3.009504 \end{array}$$

$$\underline{\underline{9.730369}} = \text{Log. Tan. } 28^\circ 15' 27''.$$

reader will conceive the line BC , which we have called the zenith-line of London, to be the pivot on which the horizontal needle ($N. S.$) is balanced; for the needle, if determined wholly by the pole of the nucleus, must necessarily take the angle of the plane which has the line BC as its common intersection with the plane of the meridian, and which passes through the point Z ; and $YK'Z$ is the angle of that plane with the plane of the meridian.

Now, the measure of that angle is easily determined by the consideration of the triangles GYK' and ZYK' , both of them right angled; for the angle FGK is known, being equal to its vertical angle BGH , which is equal to the angle of the latitude, BCM , $51\frac{1}{2}^\circ$. And the side GY in the former of these triangles is known, or may be easily determined, being equal to $GF - FY$, or $GF + FY$, according to the assumed position of Z ; while FY , as cosine, has a determinate value in relation to the radius DF . Thus, the three angles and one side being known, the side YK' may be calculated; and as this forms one side of the triangle $YK'Z$, of which at the same time the side ZY is known, from its proportion, as sine, to the radius DF , we have thus, in the triangle $YK'Z$, the two sides ascertained, enclosing the right angle. Hence, by a simple process of trigonometry, the angle $YK'Z$ is found, which is the angle sought, representing the Declination as it would be if dependent exclusively on the position of the pole of the nucleus at Z , the assumed point in its secular revolution.

It is important to observe that it is not at the quadrature that the maximum of that angle occurs; but it varies with the latitude, and at London it will be

found to be at about 65° of the revolution³, reckoning the degrees from the point of no Declination, when the pole of the nucleus was between London and the pole arctic. Nevertheless this, as we shall presently show, is, as regards the actual Declination, greatly modified by other circumstances. But, calculated as depending on the polar magnetic ray, exclusive of other modifying causes, the maximum angle of the Declination at London would, at 65° of the secular revolution of the pole of the nucleus, amount to $30^\circ 38' 24\frac{1}{2}''$, while the maximum observed angle of westerly Declination at London, as Sir David Brewster states⁴ (professedly following Colonel Beaufoy), was in the year 1815, and was only $24^\circ 27' 18''$. Dr. Ferguson, in "Chambers's Educational Course," adopts this date and amount: Mr. Brooke, in accordance with a table of Mr. Barlow's, puts it in the year 1818, and makes the amount $24^\circ 30'$, which is nearly in agreement with the more detailed facts stated by Mr. Walker⁵, on the authority of observations taken by Colonel Beaufoy at Bushey Heath, in 1817, 1818, 1819, and 1820, and published by him in the "Annals of Philosophy," which would make the maximum in 1818 about $24^\circ 35'$. The Astronomer Royal puts the maximum as probably having been in the year 1819, but makes the amount at Greenwich $24^\circ 23'$.⁶

(14.) I conclude, therefore, that the North Polar magnetic directive ray is not the sole determining cause, but that the needle is, from some one predo-

³ At Paris it would be at about 68° of the circle, owing to the difference of latitude.

⁴ "Encycl. Brit.," 7th and 8th Edd., Article "Magnetism."

⁵ "Terrestrial and Cosmical Magnetism," by Edw. Walker, M.A. Deighton, Cambridge, p. 17.

⁶ "Treatise on Magnetism," by Sir George B. Airy, 1870, p. 53.

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18' 57"

14' 14"

38' 36"

8' 12"

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64' 32"

4' 57"

minant cause, much inflected towards the axis of the earth's rotation, and also, as shall presently be shown, somewhat deflected towards the centre of the earth. There is a known and adequate cause of this, as has been intimated, which has been experimentally proved to exist; a cause which is simply the diurnal rotation of the earth: but the laws of its application in the present case, though they have already in part been proved by the experiments of Mr. Barlow, require, in relation to the earth, to be determined further by experience and observation; and, in order to the determination of them, it is necessary accurately to separate and estimate the effects, as they are to be ascribed to the one cause or the other. I subjoin, therefore, a table containing the Declination of the needle observed at London, in comparison with the Declination as it would be, if dependent solely on the direction of the pole of the nucleus, the difference between them being the amount of the inflection of the Declination attributable to the earth's diurnal rotation.

I find also, by another process, that the observed *Dip* at the western quadrature was greater than what my calculations from the position of the north pole of the nucleus would have made it by about 4° ; a difference not very dissimilar in amount from that of the *Declination* at the same point, when compared with the Declination calculated as if depending solely on the direction of the pole of the nucleus. These two differences indicate an *inflection* and *deflection* of the magnetic needle inclining in the direction of the axis of the earth or plane of the meridian, and somewhat towards the centre of the earth. The subjoined table will be found illustrative of this also. For the satisfaction of those of my readers whom it

may interest, I will briefly state the method adopted by me for the obtaining of these results. Others can easily pass over the dry account of the operations.

In regard to the Declination, I have already indicated the process by which the calculations have been made. In regard to the Dip, the calculation has been made from the angle of the magnetic polar ray with the *zenith-line* of London; by which I mean the line from the centre of the earth to the zenith of London. The Dip is the complement of that angle, or its difference from 90° ; but the zenith-line is that with which the angle of the magnetic direction is formed, and to which it may most conveniently be referred for measurement or calculation. Now, referring to the same diagram last used, if a semicircle, with diameter DE , be conceived erected perpendicularly to the plane of the paper, the London zenith-angle at any point, Z , in the circumference of the semicircle, may be determined by a process depending simply on the familiar principle, that in a right-angled triangle the square of the hypotenuse is equal to the sum of the squares of the two sides. For if from the assumed point Z in the circumference of the semicircle, ZY be drawn perpendicular to the diameter DE , then ZY is the sine of the supposed arc DZ . And if H and Z be joined by a line, then HZ^2 will be equal to $HY^2 + ZY^2$. Likewise, conceiving a line drawn from B to Z , BHZ is a right-angled triangle, and $BZ^2 = BH^2 + HZ^2 = BH^2 + HY^2 + ZY^2$; or, putting it into a more convenient form for logarithmic working, $BZ^2 = HY \left(\frac{BH(BH + \frac{ZY^2}{BH})}{HY} \right)$. On the same principle, conceiving a line drawn from G to Z , $GZ^2 = GY^2 + ZY^2$; or, adapted to logarithmic working, $GZ^2 = GY \left(GY + \frac{ZY^2}{GY} \right)$.

Thus, then, taking the square roots of these numbers for BZ and GZ respectively, the three sides of an oblique-angled triangle BGZ are found; of which BZ is the direct line from the place (London) to the pole of the nucleus; GZ the line from Z , the position of that pole, to G , the point of intersection of the zenith-line with DE produced; and BG is the portion of the zenith-line between the place, London, and the same point of intersection; the length of BG being determined by its relation to the right-angled triangle BHG , in which all the angles are known, and the side BH easily found by its proportion to the radius of the earth, for it is equal to the sine of $51\frac{1}{2}^\circ$, minus FC . Hence, the three sides of the oblique-angled triangle BGZ being found, the angles may be found by a trigonometrical process; and thus the angle GBZ is determined, which is the zenith angle of the magnetic ray from the pole of the nucleus at Z . The calculations are tedious in working, from involving so many steps dependent on one another; but this is partly compensated by the simplicity of their principle, which is more open to the general understanding than if locked in technical formulæ. I have made them with care, and have also verified them all to some extent by a simple geometrical construction and measurement. It is proper here to remind the reader that these calculations are, for the sake of simplicity, based on a sphere of equal solid contents with the earth, without taking into account the earth's actual spheroidal form; the results being sufficiently approximate for present purposes. The diameter of the sphere has been taken at 7917·2 miles; in proportion to which BH is 1456·1 miles; HF = 2464·29 miles; GF = 1306·05 miles; DF = 549·39 miles; BG = 1860·446 miles. These

are the constant quantities on which the variable ones depend; and I state them to facilitate the reader's testing of my figures.

With regard to the deflection of the Dip, a certain portion of it is simply the necessary consequence of the *inflection* of the Declination. For, retaining the same slope of the line *BZ*, the effect of diminishing the angle of Declination is to carry that line laterally inwards, nearer to the zenith-line *BK* from shortening *ZY*, and to diminish the zenith-angle, thus increasing its complement, which is called the Dip. But, after making the calculated allowance due for this, there still remains a deflection towards the centre, such as the table shows, in which these two elements will be found separated. For the purpose of this calculation the measure of *ZY*, as shortened by the inflection, is found by the proportion of the tangent of the number of degrees of the actual declination to *KY* as radius. (Rad. : Tan. Declin. :: *KY* : *ZY*). The zenith-angle, and its complement the Dip, are then calculated with this reduced measure of *ZY*, by the same process as before, and compared with the former results.

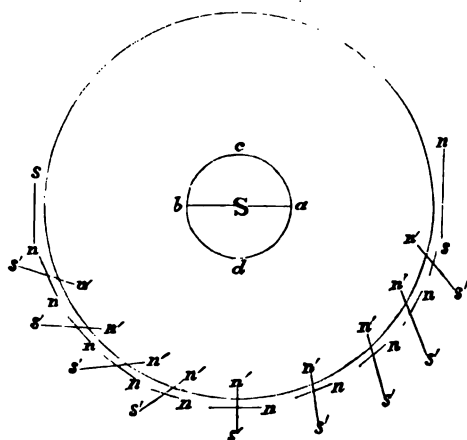
The principle which I have thus illustrated in the case of the Declination and Dip at London, admits, of course, of analogous application to any other place; which will be a work of time and combined labour, and will also doubtless show the existence of local modifying causes. But I beg the reader's attention to the fact that I have introduced no hypothetical cause, but, in respect of the inflection and deflection resulting from the earth's rotation, have only taken into practical account one known, and experimentally proved to exist; though, I believe, no previous attempt has been made to separate and calculate the

effect and manner of its operation in the rotation of the sphere. That effect cannot be left out of account any longer. To neglect it would be, not to repudiate a theory, but to close the eyes to a fact. And the manner in which it has been dealt with by me has not been by loose and vague theorizing, but by exact and careful reasoning and demonstration.

(15.) A distinct light, as to the probable cause of this inflection and deflection, is derived from Mr. Barlow's experiments on the magnetic effects produced by iron in rotation. For the body of the earth, abounding in iron ores, may be considered as so far represented in Mr. Barlow's experimental iron shell or hollow sphere: and as its rotation differs from that of the nucleus, and from the currents excited by the latter, its motion in this respect would become perceptible in reference to the needle. Mr. Barlow's experiments on this subject are related by him in a letter to J. F. W. Herschel, Esq., read in the Royal Society, May 5, 1825, and published in the "Philosophical Transactions" of that year. An account is also given of them in Sir David Brewster's article on "Magnetism," in the seventh edition of the "Encyclopedia Britannica." Mr. Barlow found that, whatever was the position of the shell's axis of rotation in relation to the needle, the north pole of the latter approached the iron sphere, when the rotatory motion of the sphere was towards the needle; and that its south pole approached the sphere when the rotation was in the direction from the needle. Having placed the axis of rotation sometimes in the magnetic meridian, sometimes in the direction of east and west, he found that whatever was the direction of the axis of rotation, the needle being always a tangent to the sphere, the north end of the needle

was attracted when the sphere moved towards the needle, and repelled when its motion was *from* the needle. The points where the sphere exercised upon it no action at all were at the extremities of the axis; and those where the effect was a maximum were at the two extremities of an axis at right angles to this. In this case the direction of the needle was towards the centre of the ball.

The accompanying diagram is given by Mr. Barlow in illustration, in which the small circle S represents the shell; $a\ b$ its axis of rotation; from c to d the direction of its rotation; $n-s$, $n-s$, the several positions of the needle prior to the motion of the shell, the needle being parallel to the tangent; and $s'-n'$, $s'-n'$, its positions assumed from the rotation of the iron sphere.



In these experiments, in order to account for and avoid certain anomalous effects, Mr. Barlow found it necessary to neutralize the effect of the earth's mag-

netism upon the needle, by magnets properly disposed for that purpose.

Now, by the light of these experiments, it plainly appears that the revolving mass of the earth, apart from the nucleus, must of necessity exert a secondary magnetic effect on the needle while it is primarily drawn towards the pole of the nucleus by the magnetic ray thence, or its equivalent in the magnetic currents caused by the nucleus. In one part of the secular revolution of the pole of the nucleus, namely, when it was passing between us and the pole arctic, this secondary effect neutralized for five years the primary attraction, there having been no declination from 1657 to 1662, though in that space of time the revolution must have proceeded probably nearly three degrees, or about $2^{\circ} 50'$ of the circle. But, in the first place, before the pole of the nucleus reached the position of due north, the needle was drawn inward to it by the magnetic effect of the earth's rotation; and again, after it had passed that position, the needle was held to it by the same cause, until the pole had proceeded some distance on the path of its slow revolution, and the angle caused by it had become more considerable. Thus a remarkable feature of the case is accounted for.

Mr. Barlow, though disappointed of finding in his experiment of the revolving iron shell any explanation of the main primary cause of the earth's magnetism, nevertheless expressly states his conviction that it involves a principle which, under certain conditions, must materially affect the earth's magnetism. "One of the essential conditions," says he, "for the production of such an effect has place in the earth, namely, that it does not revolve about its polarized

axis; and if the inductive principle through which it receives its magnetism be exterior to itself, then it would follow almost of necessity that some such effect would take place." Now it is plain that the source of the earth's magnetism is extraneous to the main revolving mass of the earth, equally whether it be exterior or interior, in the height above or in the depth beneath. The statement which has been just quoted from Mr. Barlow applies in its full force to the case as I have represented it.

(16.) Moreover, as the revolving parallels of the nucleus, as viewed from any given place upon the earth, will meet the revolving parallels of the earth at a very different angle to the meridian, when the pole of the nucleus is on the east side of the meridian of the place, from what they would when the same pole was on the west side (the angle at the one quadrature being a right angle *minus* $18\frac{1}{2}$ degrees, and at the other a right angle *plus* $18\frac{1}{2}$ degrees, and varying in intermediate positions, somewhat as does the inclination of the ecliptic to the parallels of latitude, but giving in the whole a difference of 37 degrees between the extremes), so also the magnetic needle at the given place, as held to its position towards the pole of the nucleus, will lie differently towards the revolving sphere of the earth in the one case and in the other, and will be crossed by the parallels of the earth's rotation at a very different angle, most of all different at the eastern and western quadratures of the secular revolution of the magnetic pole. Consequently the inflection and deflection arising from the cause we have now been referring to, will not be symmetrical in quantity in the easterly and westerly variation of the needle as compared with one another, but may be expected to be greater when

on the one side of the meridian than on the other. And so, accordingly, the facts prove the case to have been. Thus, eighty-four years before the needle arrived at the central line of no horizontal variation, the Declination in 1576 was $11^{\circ} 15'$ east, and the Dip $71^{\circ} 50'$: but, eighty-four years after passing the point of no variation, the Declination in 1744 was about $16^{\circ} 56'$ west, there being a difference from the symmetrical position ($11^{\circ} 15'$) of as much as $5^{\circ} 41'$; and the Dip also, after passing the maximum, did not recede in the same time to the same measure of $71^{\circ} 50'$, but was then about 2° more, and reached the nearly symmetrical amount of $71^{\circ} 53'$ not till forty-six years later, in 1790. Thus the inflection towards the axis or plane of the meridian, and deflection towards the centre of the earth, were much greater when the Declination was easterly ; and this inflection of the easterly horizontal Declination was in quantity not very remote from what is shown in the diminished angle of the magnetic equator with the earth's equinoctial line, which, instead of the full amount of $18\frac{1}{2}^{\circ}$, shows, it is stated, only between twelve and thirteen degrees. Probably this last is principally attributable to the same cause. And to the same, in part if not in whole, may be ascribed the unequal distance of the two nodes of the magnetic equator described in the following passage of Baron Humboldt :—"The nodes of the two equators, that is to say, the two points at which the line without inclination intersects the terrestrial equator, and consequently passes from one hemisphere into the other, are so unequally placed, that, in 1825, the node near the island of St. Thomas, on the west coast of Africa, was $188\frac{1}{2}^{\circ}$ [instead of 180°] distant from the node in the South Sea, close to the little islands of Gilbert, nearly in the meridian of

the Viti group'." It may be mentioned that the deflection is liable to a slight modification by the *annual* and diurnal variation. For Professor Hasteen, as it is stated by Sir David Brewster^a, observed with a dipping needle made by Dollond, that the Dip in summer was about 15 minutes greater than in winter, and about 4 or 5 minutes greater in the forenoon than in the afternoon. But these things are now in process of being more accurately and more constantly observed by the aid of the self-registering apparatus.

Further, the effect arising from this cause on the two sides of the magnetic equator, and in the corresponding parts of the northern and southern hemisphere, will not be symmetrical; because the revolution of the earth being in the same direction, the south pole of the needle would be repelled instead of being inflected and deflected by it. Whether the amount of the repulsion in the southern may be assumed equal to the inflection and deflection respectively in the northern hemisphere, we want the test of experience. It would seem as if it should be added not subtracted in the Declination, and subtracted not added in the Dip; and the same rule of contrary might be supposed to hold as to the respective degree of the repulsion, as it affects the easterly and westerly declinations. But it is difficult to estimate the practical effect which would follow, the south pole of the magnetic needle being attracted by the magnetic pole, but repelled by the rotation. The result, as regards the Dip, would probably be to throw the south pole of the dipping needle somewhat

^a "Cosmos," i. p. 177.

^a Article "Magnetism," "Encycl. Brit.," 7th Edit., p. 746.

up, and cause it to point as if to a remoter position, while the obliquity of its direction in reference to the plane of the meridian would cause the apparent remoteness to affect the longitude of the position supposed to be indicated.

(17.) But neither would this suffice to account for the observed facts in the south. Still other causes must, I think, be taken into account to explain the magnetic Declination of the needle in the southern hemisphere, unless it should be found that the intersecting curves of the magnetic action from the south magnetic pole, and from the diurnal rotation of the earth, produce some such reversal of position in the needle as was observed by Professor Robison in certain other circumstances ¹.

To take some instances. At present the Declination of the needle at Cape d'Agulhas, Cape of Good Hope, is about 30° west of south; but if a magnetic ray were extended to Cape d'Agulhas from the point which is antipodal to the north magnetic pole, its Declination at the Cape would have been 23° to the *east* of south. There is thus a deviation amounting to 53°. Again, taking the Declination observed at two places at sea by Sir James Ross in his antarctic voyage; at the first of these, in lat. 64° 24' S., long. 148° 27' E., the needle pointed 16° 41' *east* of south. Now rectifying the globe for this latitude, bringing the given longitude to the zenith, fixing the quadrant of altitude over it, and taking its graduated edge to the place of the south magnetic pole, assumed anti-

¹ See a valuable article on "Magnetism" by Dr. Robison, with an Appendix by Professor Playfair, in the Supplement of the third edition of the "Encyclopedia Britannica," in which he details and explains some very curious effects of the action of two magnets upon a third placed in certain positions. No. 29, p. 122.

podal to the northern, it cuts the horizon about 43° to the *west* of south ; a difference of about $59^{\circ} 41'$. At the other place, lat. $65^{\circ} 4' S.$, long. $142^{\circ} 49' E.$, the Declination was $12^{\circ} 37' E.$; now, using the globe in the same manner as before, the Declination of the pole antipodal to the north magnetic would be about 51° , giving a difference of about $63^{\circ} 37'$. It is also worthy of observation that if the lines of direction of the Declination at these two last places be drawn on the globe, duly rectified for the purpose at each place, they are parallel, not converging. And this circumstance of itself would seem to indicate that these two lines are not lines conducting even in any approximate degree to the magnetic pole (otherwise in that high latitude they must have shown some convergence), but that they are more probably in this instance parallels, obliquely transverse to the converging lines. Other examples of a similar nature may be taken from the observations made in the same antarctic expedition. Thus between November, 1840, and April, 1841, (1) on board Her Majesty's ship *Terror*, in lat. $54^{\circ} 5' S.$, longitude $134^{\circ} 44' E.$, the Declination was $3^{\circ} 1' E.$ On similarly rectifying the globe, and drawing the line of direction by the quadrant, it will be found nearly parallel to the two last mentioned. (2) On board the same ship, in latitude $55^{\circ} 5' S.$, longitude $132^{\circ} 7' E.$, the Declination was found $0^{\circ} 59' W.$ On applying the same method this line of direction appears not only not convergent, but slightly divergent from the three preceding. (3) In the same ship, in lat. $56^{\circ} 39' S.$, long. $129^{\circ} 45' E.$, the Declination was $6^{\circ} 13' W.$; a direction which, on the same trial, will be found more divergent. (4) On board Her Majesty's ship *Erebus*, in the same voyage, in lat. $56^{\circ} 14'$, long. $130^{\circ} 44' E.$ the

declination was $5^{\circ} 17'$ W., giving a line of direction nearly parallel to the last. In the higher latitudes reached, the divergence becomes even more conspicuous, nay extreme. Thus (5) on board the *Erebus*, in lat. $76^{\circ} 23'$ S., long. $177^{\circ} 25'$ E., the Declination was $84^{\circ} 55'$ east; and (6) on board the same ship in lat. $77^{\circ} 52'$, long. $189^{\circ} 27'$ E., the Declination was $93^{\circ} 31'$ east; giving lines of direction diverging from one another on the globe, and extremely diverging from the others above specified². Referring to these by the numbers which I have prefixed to them for the purpose, the probable position of the pole of the axis of the earth's nucleus would give the several angles approximately though roughly by the globe somewhat as follows:—

(1) $33^{\circ} 18'$ west; difference $36^{\circ} 19'$. (2) $33^{\circ} 45'$ west; difference $32^{\circ} 46'$. (3) $35^{\circ} 45'$ west; difference $29^{\circ} 32'$. (4) $35^{\circ} 45'$ west; difference $30^{\circ} 28'$. (5) $52^{\circ} 30'$ west; difference $137^{\circ} 25'$. (6) $47^{\circ} 30'$ west; difference $141^{\circ} 1'$. In the attention given by philosophers to the convergence of the *Isogonic lines*, the fact to which I have been adverting, viz., the absence of convergence and existence of divergence in the lines of direction of the Declination in the south, seems to have escaped notice; at least I have not seen it remarked, though it is worthy of it. Again, at Hobarton, lat. $43\frac{1}{4}^{\circ}$ S., long. 147° E., the declination of the needle in 1843 was $9^{\circ} 53'$ E., and in 1848, $10^{\circ} 0.6'$ E.; while, if we refer to the globe in the same method, the southern point antipodal to the

² The six observations just cited are taken from the "Philosophical Transactions" for 1868, from the Tables there given of the results of the magnetic survey of the South Polar regions of the globe, with General Sir Edward Sabine's review of them, p. 389, &c.

north magnetic pole, would be, in the former case, about $27^{\circ} 10'$ *west*, and in the latter about 27° *west*, giving a difference of about 37° .

I have applied the same method to various other southern observations³, and find the same result;

³ Among others, the following from Dr. Halley :—

	Year.	Declination.	Direction of the S. M. Pole (assumed antipodal to N.M. Pole at that time.)	Difference.
At Cape d'Agulhas, Cape of Good Hope . . .	1622	$2^{\circ} 00' W$	about $5^{\circ} 30' E$	$7^{\circ} 30' W$
	1675	$8^{\circ} 00' W$	„ $9^{\circ} 00' E$	$17^{\circ} 00' W$
Magellan's Straits— West entrance. Long. $75^{\circ} 0' W$ from Par.	1670	$14^{\circ} 10' E$	„ $26^{\circ} 00' W$	$40^{\circ} 10' E$
Lat. $53^{\circ} 0' S$. . .				
East entrance. Long. $68^{\circ} 00' W$ from Par.	1670	$20^{\circ} 30' E$	„ $24^{\circ} 00' W$	$44^{\circ} 30' E$
Lat. $52^{\circ} 30' S$. . .				

In the case of the smaller angles above, the magnetic pole was at the remoter side of its orbit round the geographical pole relatively to the place.

To these may be added the following from the collection made by Messrs. Mountaine and Dodson in the “Transactions of the Royal Society,” showing, in most instances, declination to the contrary side :—

At Sea, Lat. $35^{\circ} S$.	Year.	Declination.	Direction of the South Magnetic Pole (assumed antipodal to North Magnetic Pole at the time).	Difference.
Long. 0°	1700	$2\frac{1}{2}^{\circ} W$	about 2° or $2\frac{1}{2}^{\circ} E$	5°
„ $5^{\circ} W$	1700	0°	„ $0\frac{1}{4}^{\circ} E$	$0\frac{1}{4}^{\circ}$
	1744	$5\frac{3}{4}^{\circ} W$	„ $14^{\circ} E$	$19\frac{3}{4}^{\circ}$
	1756	$7\frac{1}{4}^{\circ} W$	„ $15\frac{1}{4}^{\circ} E$	23°
„ $10^{\circ} W$	1700	$2\frac{1}{4}^{\circ} E$	„ $2\frac{1}{2}^{\circ} W$	$4\frac{3}{4}^{\circ}$
	1744	$3\frac{1}{4}^{\circ} W$	„ $12\frac{1}{4}^{\circ} E$	$15\frac{1}{2}^{\circ}$
	1756	$5\frac{1}{4}^{\circ} W$	„ $14^{\circ} E$	$19\frac{1}{2}^{\circ}$

that they give the Declination often quite to the other side of the geographical pole from that on which the point antipodal to the north magnetic pole would be; and they appear to me not to take a direction even approximately converging to any position of the south magnetic pole, but obliquely transverse to the converging lines, and that often at a great angle. In short they seem to indicate the intervening of a diamagnetic force greatly modifying the results, and probably combined with the earth's rotatory force, repellent of the south pole of the needle, becoming increasingly predominant in the higher southern latitudes, as the horizontal directive force of the south pole of the nucleus becomes weaker.

(18.) And why should it not be so? If Professor Barlow has demonstrated that a revolving sphere of iron, which is a paramagnetic substance, produces

At Sea, Lat. 32° S.	Year.	Declination.	Direction of the South Magnetic Pole (assumed antipodal to North Magnetic Pole at the time).	Difference.
Long. 15° W	1700	4½° E	about 4½° W	9½°
	1744	0½° W	„ 11° E	11½°
	1756	2½° W	„ 12½° E	15°
„ 20° W	1700	6½° E	„ 6½° W	13°
	1744	2½° E	„ 10° E	7½°
	1756	0½° E	„ 11° E	10½°
„ 25° W	1700	8½° E	„ 8° W	16½°
	1744	5° E	„ 8½° E	3½°
	1756	3° E	„ 9½° E	6½°
„ 5° E	1700	5° W	„ 3° E	8°
	1756	13° W	„ 17° E	30°
„ 10° E	1700	7½° W	„ 5½° E	12½°
	1756	15½° W	„ 19° E	34½°
„ 15° E	1700	9½° W	„ 7° E	16½°
	1756	17½° W	„ 20½° E	38°

paramagnetic effects, why should not a revolving sphere of a diamagnetic substance produce in its degree diamagnetic effects? In the southern latitudes there is the revolution of a large segment of the globe, of deep and uninterrupted water: and water has been demonstrated by Faraday to be diamagnetic, as are also some of the salts with which it is impregnated. By different forms of experiment it has been proved that water placed between two powerful magnets tends to assume a position at right angles to their line of junction, or, as it has been termed, places itself equatorially not axially. Professor Tyndall also has experimentally demonstrated the polarity of the diamagnetic force in a variety of diamagnetic substances, and, among the rest, in pure water⁴; and by artificially increasing the diamagnetic force a magnetic needle was deflected. But it is a universally received principle that action and reaction are equal; and, though the reaction of the diamagnetic force of the water upon either of the magnets was, in the experiment, of course utterly inappreciable, yet, when the revolving diamagnetic mass is made immeasurably vast, and the paramagnetic force of one of the two magnets between which it is interposed, namely, the needle, is in itself very small, it cannot be a subject of wonder that it is, in this case, the needle that is moved by the diamagnetic force, and not the whole mass of the ocean by the paramagnetic force of the needle. Nevertheless, the power of the earth's polar magnetism, or, in other words, the polar magnetic effect of the nucleus is shown in drawing the needle into the oblique position, generally inclining towards the

⁴ "Phil. Trans.," vol. cxlvi. (for 1856), page 251.

polar quarter, though at a large angle to the true pole of the magnetic nucleus. In short, the position of the needle is the resultant of the two forces.

Although the application of this principle to the explanation of the facts has not till now been made, and therefore needs further investigation, yet the fact of an influence of the ocean upon the magnetic needle has not passed unremarked. "We observe," says Baron Humboldt, "that the isogonic curves when they pass in their secular motion from the surface of the sea to a continent or island of considerable extent, continue for a long time in the same position, and become inflected as they advance⁵." It may be remarked further, that it has been thought that, in electro-magnetic currents, repulsion may arise from the peculiar action of a fluid on a solid part of the circuit⁶. How much more may a boundless ocean quite encircling the revolving globe have some such power on the magnetic needle. There are also other circumstances, long known, which ought not to be left out of consideration in regard to the diamagnetic force. Take the following from Dr. Robison. "As red-hot steel acquires permanent poles by quenching it near a magnet, so melted wax (a diamagnetic body) acquires them by freezing in the neighbourhood of a positive and negative electric⁷." From the analogy of magnetism with electricity a question is also suggested by some experiments of Dr. Priestly, in which he found that along the surface of different bodies a battery may be made to discharge itself at a much greater dis-

⁵ "Cosmos," vol. i. p. 174.

⁶ Ferguson, "Electr. and Magn.," pp. 174. 109.

⁷ Article "Magnetism," Supplement to 3rd ed. of "Encycl. Brit."

tance than it otherwise would. The experiments were begun with ice; and he first accidentally discovered that when the shock of a common jar was discharged on a sheet of ice, it would sometimes run over the surface and strike the chain directly on the other side. Ice at 32° of Fahrenheit is a semi-conductor of electricity; at extreme polar temperatures a non-conductor. Now there is no reason to think that there is any open sea at the south pole, as is thought to be the case at the north, and the ice extends unbroken to a much greater distance from the pole. Whether this may make any difference in the transmission of superficial or other currents, I do not profess to determine. But it appears to me that every circumstance of difference should be noted.

(19.) But to return to the general law. It may be observed that the effects specified indicate an angular difference of the revolution of the nucleus from the revolution of the earth, and go to prove it. It appears to me also that this angular position of the axis of the rotation of the nucleus in relation to the axis of the earth, while both coincide and cross in a common centre which is the centre of the earth, may probably be found to have something to do with the singular conclusion to which the mathematician Biot was led, on comparing the results obtained by computation from the theoretical formulæ with the observed phenomena, that, to reconcile the differences tolerably, the contrary poles or directive forces should be assumed to be infinitely near to one another and to the centre of the earth^a. The Astronomer Royal, at page 103 of his "Treatise

^a See Appendix C.

on Magnetism," has presented a succinct view of this theory first suggested by Mayer; and he remarks that its results, though in general agreement with the broad facts of terrestrial magnetism, failed in accuracy when applied to many special cases. He also observes upon it, "These three results, for horizontal force, for dip, and for total force, are not materially disturbed if we conceive the magnet to be excentric, provided that magnetic latitude is always referred to its centre." Why magnetic latitude must be referred to the centre is sufficiently evident on the view which we have been endeavouring to illustrate.

It is desirable that it should carefully be observed whether in high northern latitudes any effect takes place as regards the inflection similar to what was found by Professor Barlow, in the experiment of his iron shell, between the pole of the axis of rotation and the thirtieth degree measured thence. What he has stated in regard to that peculiar feature of his experiment deserves fuller attention: it were to be wished that it might be more clearly accounted for; though it has been shown by him to be in accordance with the "*conjectural law*" (as it has been justly called by the Astronomer Royal^o), that the tangent of the dip is equal to twice the tangent of the magnetic latitude.

(20.) On the Tables which I have submitted I may remark that they present results which differ in some respects from what I had expected, but which appear to me so much the more instructive from their agreement with known laws which I had not anticipated the exemplification of here. This is peculiarly the case in the column in which I have given the

^o "Treatise on Magnetism," p. 97.

proportion of the inflection to the calculated angle of declination. A perfectly regular series of numbers was not to be looked for, owing to the nature of the data as to the actual declination, and the various modifying circumstances; but one thing is conspicuous, namely, the great and continuous decrease of the proportion of the inflection relatively to the angle of declination. At $3\frac{1}{2}$ degrees in the secular revolution of the magnetic pole (A.D. 1666), the proportion of the inflection to the magnetic polar angle of declination was as 8249 to 10,000; whereas at 120° A.D. 1872, if I correctly estimate the time), it will be reduced to the proportion of 672 to 10,000. Now if this be rightly considered, it will be seen that it is dependent on the relative *intensity* of the magnetic forces, and is not indicative of a decrease of the inflecting force, but only equivalent to a relative continuous rise in the proportion which the horizontal force of the declination bears to the force inflecting towards the plane of the meridian. Whether that continuous rise in the *proportion* of the former to the latter is to be attributed to an increase of intensity in the former, or a decrease in the latter, facts must determine; and they do determine it in full harmony with the relative change of proportionate intensity in the table. The inflecting force is dependent on the rotation of the earth, and at a given latitude and place it may be expected not to vary, certainly not to any such extent: but the distance of the pole of the nucleus from the point of observation, London, has increased and is increasing, in the progress of its secular revolution. We have then a necessary change of our *magnetic* latitude continuously going on; and that change of latitude is not slight, but will amount when at the

maximum to 37 degrees. In other words, our magnetic latitude has continuously been becoming more southerly from the time when the needle passed the point of no declination, and will go on becoming more southerly till the magnetic pole has accomplished one-half of its secular revolution, and so has come round to the plane of the meridian on the other side of the pole arctic. Now it is a well-known and established law, that the horizontal magnetic intensity is greater as we are nearer to the magnetic equator; while on the contrary the vertical and general intensity is less¹. But it is the horizontal intensity which we have to do with in

¹ It may be satisfactory to the reader to have authority for this statement, and I am enabled to give him that of the Astronomer Royal:—"Proceeding in either direction, north or south, from this equatorial belt, the *Horizontal* Force gradually diminishes to 0 at each pole."

"There is a rude approach to the law that the *Total* Force at the magnetic poles is double that at the magnetic equator."

"At the end of Article 41 it was remarked that at Greenwich, the dip and total force are diminishing. Interpreting these by the remarks above, it would seem that *the magnetic equator is approaching to Greenwich, or the north magnetic pole is receding from Greenwich*. And, remarking also the westerly change in direction of the north magnetic meridian, from the sixteenth century to the year 1824, and its subsequent easterly motion, it would seem that the north magnetic pole has rotated round the terrestrial pole in a small circle from east to west, and having passed the point where its westerly azimuth, as viewed from Greenwich, is maximum, it is still continuing its course in that circle. It seems probable that in the fifteenth or sixteenth century it was situated between North Cape and Spitzbergen. It is now north-west of Hudson's Bay."—Sir George Biddell Airy's "Treatise on Magnetism," 1870, pp. 97, 98.

In regard to the Total Force it is necessary to add that the *maximum* is not at the magnetic poles. The point of greatest intensity in the northern hemisphere is situated to the west of

measuring the proportion of the inflecting force to the calculated polar angle of declination. Since, then, the terrestrial latitude of the place has been constant, but the magnetic latitude has been continuously becoming more southerly as the pole of the magnetic nucleus moved farther off, there must of necessity take place, according to this known magnetic law, that relative increase of the horizontal intensity in the declination as compared with the inflecting force depending on the earth's rotation, which the Table so prominently shows. Consistently with this it is an admitted and well-known fact, that at present the horizontal intensity in Europe is increasing².

Since writing these pages and constructing these Tables, I have had the opportunity of consulting the Astronomer Royal's valuable "Treatise on Magnetism," published subsequently to Nov. 1870³, and am much pleased to observe there (pages 74, 75) that this rise of the horizontal force has been proved and measured by actual experiment and observation; of the method of which an interesting account is given (pages 53 et seq.): and in the course of nineteen years from 1848 to 1867, the increase was in the proportion of 29 to 30. This, it will be observed, is the increase of the horizontal force as a whole; whereas the proportions in my Table represent the change in the relative amount of the two principal constituent parts of which it is the result.

(21.) But further, the Table thus brings to light

Hudson's Bay, some 18° south of the magnetic pole.—Ferguson's "Electricity and Magnetism," Gen. Sabine's Chart, and the Astronomer Royal's Treatise, p. 98.

³ Ferguson's "Electricity and Magnetism," p. 34.

² The preface and dedication are so dated.

the reason why the needle continued for several years to point due north without variation; namely, at London, from 1657 to 1662. For the proportion of the inflecting force was then so high as to be equal to the force of declination. At Paris the same effect took place, and perhaps for a still longer time; for though the longitude of Paris is $2^{\circ} 22'$ to the east, yet no westerly declination of the needle was shown there for a year or two later than at London⁴. As the latitude of Paris is $48^{\circ} 49'$, while that of London is $51\frac{1}{2}^{\circ}$, both the inflecting force and the lateral intensity of declination may be greater at the former. And as at Paris the angle of declination, measured towards the pole of the nucleus, is less, as may be shown on the same geometrical principles,

⁴ Humboldt says, "*twelve years later*," reckoning erroneously from 1657 to 1669; but the needle did not leave the point of declination at London till 1662, while it was at the point of no declination at Paris probably by, or about 1664, in which the declination there is recorded by some $0^{\circ} 4' E.$, but certainly before the year 1666, at which time the declination there is recorded by Halley, on the authority of Orontius Finæus, to have been $0^{\circ} 0'$, while in 1667 it is given by Sir David Brewster $0^{\circ} 15' west$, on the authority, I believe, of Picard, who is cited by Mr. Walker as having recorded that amount. In 1670 it was $1^{\circ} 30' west$; in 1680, $2^{\circ} 40'$, according to Picard. (See "Encycl. Brit." 7th and 8th editions, and "Terrestrial and Cosmical Magnetism," by Edward Walker, M.A., the latter work highly commended by Dr. Airy.) I know not whether some continental attraction, similar to what has been alluded to above (p. 165), may have been added to the general causes which influenced the result: for it appears as if the *easterly* declination continued longer at Paris than at London, if the observations were correctly made, and have been correctly recorded. But it is to be taken into account that to the English, as a more seafaring people, the accurate observation of the declination was of more consequence than to the Parisians; and that the Paris statements of the declination, at that period, possibly may thus be somewhat less reliable.

this would account for the inflecting force maintaining its predominance or equality for a longer time there, so keeping the needle longer to the due north. I find on calculation that the proportion of the inflection to the declination has been uniformly considerably greater at Paris than at London. Thus at Paris in 1814, at 90° of the magnetic pole's secular revolution relatively to that place, the calculated angle of horizontal declination towards the pole of the nucleus would be $26^\circ 56' 8''$; but the actual declination was $22^\circ 54'$; giving for the inflection the amount of $4^\circ 2' 8''$; whereas at the corresponding point of the secular revolution (at 90°) in relation to London, the calculated angle is $28^\circ 15' 22''$, the observed declination $24^\circ 23'$, and therefore the inflection was $3^\circ 52' 22''$; a much smaller proportion to the larger angle. The same is still more conspicuous on comparison of the earlier stages of the revolution.

It is worthy of remark that while the needle at Paris was a year or two later of leaving the due north, it turned at least a year, and, according to some reckonings, four years, earlier than at London from the point of maximum westerly declination. The latter circumstance so far accords with the easterly longitude of Paris: the former was an apparent anomaly, but meets partially with its solution now.

(22.) In regard to the dip it is at once evident that the deflection towards the centre would cause the needle to be vertical at a slightly lower latitude than the real pole of the axis of the nucleus, and accounts for its being observed in latitude $70^\circ 5'$, instead of $71^\circ 30'$ north.

In regard to the very perceptible and gradual diminution of the deflection after passing the quad-

rature which is shown in the last column of the Table, I think it is to be attributed to the influence of the south magnetic pole upon the needle becoming gradually more sensible. For up to near 80° of the secular revolution, the magnetic latitude of London was higher than its geographical latitude; but then it became equal; and since that time lower; and is still gradually becoming lower, as the north magnetic pole is steadily moving farther away, while at the same time the angular horizontal declination, and still more the inflection, are becoming less.

Whether this increasing influence of the south magnetic ray may by and by, when the north magnetic pole approaches its extreme of distance from us, be sufficient to do more than counterbalance the central deflection, so as even to diminish the dip below what I have in the Table indicated as the minimum of the direct angle to the north pole of the nucleus, time and observation will show. But the fact that it is not yet equal to it, shows that no material error has resulted from the method which has been pursued, by which these several powers have been analyzed and separately brought out, in a form which I hope may render them capable of distinct comparison, and eventually of more accurate determination.

(23.) The existence of a relation between magnetism and gravitation has already been alluded to, as well as the impression entertained by some scientific men that gravitation would eventually be found to resolve itself into a resultant of the same power. However far the two may yet seem from identification, there are points of very remarkable correspondence which have presented themselves, among which is the law, which may be considered now fully and

demonstrably established⁵ in regard to magnetism as well as gravitation, that the force is inversely as the square of the distance; together with the remarkable similarity in both as regards the solar and lunar influence in the production of tides. I recur to the subject now in order to remark one further point in which the analogy of the two may suggest the application of a principle to magnetism which is known to hold in reference to gravitation. It is well known that the centrifugal force in the rotatory movement of any body counteracts to a certain extent the central attraction of gravity, and is therefore in one view equivalent to a diminution of it. Now in the rotation of a sphere this centrifugal or tangential force diminishes as we recede from the equator, and is nothing at the poles. It is also experimentally proved that rotatory movement affects the magnetism of an iron sphere, and that this peculiar effect is nothing at the poles of the axis of rotation. It is moreover the tangential force which produces the peculiar effect, for it is reversed according as the sphere revolves towards the needle or from it. Considering then that the nucleus of the earth is rotating, and that its rotatory centrifugal force diminishes in the direction of its poles, and considering that the point in each hemisphere where the central attraction of the nucleus is undisturbed and undiminished by this tangential force is the point corresponding to the pole of its axis produced, this may give rise to a directive force inclining towards that pole, and which would act vertically downward at that point. This principle is to be distinguished from that of the generating of electric currents formerly adverted to;

⁵ Sir George Biddell Airy's "Treatise on Magnetism," p. 65.

but both principles must be regarded and taken account of; and it is a recognized fact that the dipping needle is at right angles to the direction which earth-currents of electro-magnetism tend to assume⁶.

(24.) In regard to the part which electric currents may have in magnetism, Ampère showed that a coil or helix of wire, through which an electric current is passed, will show the properties of a magnet having its two extremities as north and south poles; and he proposed a theory according to which magnetism is no longer to be regarded as distinct from electricity, but resolves itself into it. The magnetism of the earth itself was explained by him as resulting from currents of electricity constantly traversing it in a direction from east to west. Sir Humphrey Davy, in a letter on the magnetic phenomena produced by electricity (read November, 1820) published in the "Philosophical Transactions," vol. cxi., mentions in note some circumstances which are unfavourable to Ampère's theory of the identity of electricity with magnetism, but avoids pronouncing upon it. On these matters, and on this theory, I may refer the reader to Mr. Brooke's "Elements of Natural Philosophy'," where they are briefly and clearly stated and illustrated. Mr. Brooke remarks, "It is unquestionable that, opposed as this view is to all previously received conventional theories, it is entirely in accordance with every discovery in electro-magnetic induction, as well as with all other observed facts in electricity and magnetism." The cause, indeed, to which Ampère ascribed the origin of these electric currents, namely, the supposed chemical effect

⁶ Ferguson's "Electricity and Magnetism," p. 176, No. 112.

⁷ Ed. 1867 (J. Churchill & Sons, London), Nos. 873—884, especially 883-4, p. 496.

of a nucleus consisting of those metallic bases of the earths which were discovered by Sir Humphrey Davy, acting upon the surrounding oxidized layer, has been justly remarked by Humboldt to be untenable, on the ground of the great mean density of the earth (5.44) when compared with the specific weight of potassium (0.865), of sodium (0.972), or of the metals of the earths (1.2)^a. Again, the fact that electric currents of a superficial nature on a sphere will produce effects upon the magnetic needle similar to the actual phenomena has been experimentally demonstrated by Barlow; but the Astronomer Royal has shown that the cause of terrestrial magnetism cannot be superficial, but must be very deep^b. But it is in full consistency with these weighty opinions, that a nucleus, revolving upon an axis angularly inclined to the axis of the earth's rotation, would generate deep electric currents, between the nucleus and the interior hollow sphere of the earth within which it moves. Nor is it unlikely that those currents should be from east to west, though the rotation both of the nucleus and of the earth be from west to east; because the currents would arise not from the rotation of the two, so far as they concurred in the eastward direction; but from the difference of the two rotations. It is difficult at first to form a distinct idea of the effect of the revolution of a spherical nucleus in the hollow interior of a revolving sphere, inclined at an angle to its axis of rotation. But if the reader take an artificial terrestrial globe, and elevate the north pole $18\frac{1}{2}$ degrees above the north point of the horizon, and, having fixed the quadrant of altitude at the

^a Humboldt, "Cosmos," vol. i. p. 187 note, and p. 234.

^b Sir George Biddell Airy's "Treatise on Magnetism," p. 101.

zenith, conduct it to the east point of the horizon, the globe may then, for the time, be taken to represent the nucleus, and the wooden horizon, together with the brass meridian and the quadrant of altitude, may furnish a sort of rough skeleton of the sphere of the earth within which the nucleus moves. Select then some point on the globe under the brazen meridian a little to the north of the equator, and, keeping an eye on that point, let the globe be moved one quarter of a revolution on its axis towards the east (the quarter revolution may be measured by the hour circle). The point of the supposed exterior sphere which was at the zenith being assumed to have revolved in the same time, in the direct line of the quadrant, to the east point of the horizon, the previously selected point on the globe will have passed obliquely under the quadrant, at an angle inclining towards the north of east. In the opposite part of the revolution it will incline towards the south of east. But if, now, the rate of rotation of the free nucleus be assumed more rapid, on the recognized principle that the time of a revolution is in proportion to the volume of the revolving body, then the rotation of the main body of the earth, falling *relatively* behind, or to the west, may thus produce a westerly magnetic current upon the nucleus. The secular revolution of the magnetic pole is also, as has been remarked, in the westerly direction¹.

(25.) There is a further fact regarding the magnetic intensity which, though it may not be fully explained, may be noted in this place, as it offers one singular point of suggestive correspondence. The maximum total intensity in the north-western regions

¹ Above, pp. 119, 120, and Appendix A.

is not where the dip is vertical ; but it was found to be in North America about 18 degrees south of the magnetic pole, and to the west of Hudson's Bay². Now as the magnetic pole is in that estimate assumed to be between 19° and 20° from the geographical pole, (which is slightly more than its real distance), we shall not be far wrong in concluding that, taking the magnetic pole at 18½° distance from the pole arctic, the place of maximum intensity is in that parallel of *magnetic* latitude which passes through the pole arctic, and is at that part of the parallel of magnetic latitude which is furthest from the pole arctic. In reference to this it may be remembered, in Mr. Barlow's experiment, that at the latter pole, answering to the pole of rotation of his iron shell, is the place at which the *inflecting* force would be absolutely nothing. If, then, the effect of polar declination be produced, as on Barlow's theory, by a circling current of electricity, that current, in the circle of that parallel of *magnetic* latitude, passes through the point at which the counter magnetic effect of the earth's rotation is nil, namely, the geographical pole. It also intersects the corresponding parallel of *arctic* latitude in two points, each of which is 120 degrees from the place which is said to be that of maximum intensity. Thus it seems to be at the farthest remove possible from the counteracting influence ; and it may be on this account that the intensity is found the greatest.

(26.) I have not thought it necessary to enter here into any consideration of the theories of Hansteen or of Gauss. Of the former the Astronomer Royal has

² See General Sir Edward Sabine's Dynamical Chart, in Keith Johnston's "Physical Atlas," and Ferguson's "Electricity and Magnetism," p. 32.

remarked that "nothing has resulted from his theory." Of the latter he speaks in highly complimentary terms; and it may be added that we are much indebted to Lady Sabine for the English translation of his difficult and elaborate treatise, which was published by her in the second volume of Taylor's "Scientific Memoirs," having previously received the careful revision of Sir John Herschel. This theory, assuming the general but irregular distribution of magnetism in the earth, aims at determining how it will act at different points on the earth's surface, from having in each case the magnetic elements of eight stations given at a sufficient distance from each other. Its office was thus an attempt to supplement observation, at those places where it could not be or had not been obtained, by probable deduction based on mathematical principles. Its publication strengthened the grounds on which a desire was felt for more complete and trustworthy data than were possessed by M. Gauss in 1839. But on the subsequent scientific expedition of Sir James Ross to the antarctic regions, it was found that the theory failed to reproduce in the southern hemisphere the peculiar and characteristic features of the terrestrial magnetism which were so well established by observation, and tolerably represented by the theory in the northern hemisphere. The result of the calculations was "found to differ widely from the facts made known by the survey". "The strong objections," observes Sir Edward Sabine, "to receiving on less than conclusive evidence so improbable an anomaly as this diversity in the two hemispheres, tended,

* Gen. Sir Edward Sabine, Pres. Royal Soc., "Phil. Trans.," 1868, p. 381.

without doubt, greatly to stimulate the endeavours of those who desired to render the facts of observation in the southern hemisphere more commensurate with the theory in which they were employed." Ascribing this failure of the theory, as it had been provisionally applied, to the deficiency of data furnished by the previous observations, General Sir Edward Sabine has, in the "Philosophical Transactions" for 1868, supplied fresh numeral coefficients for the application of the theory; but the result of these remains to be tested. A lesson of caution has been learned as to the degree in which it can be trusted in advance of observation; and yet it appears to be only in the laborious approximate adjustment of probable details to supply deficiency of observations that its value consists. The secular variations of the needle are wholly unaccounted for by it. The anomalous diversity in the two hemispheres shows that the physical cause cannot be assumed to be such a general though irregular distribution of magnetism in the earth as the theory supposed; and as far as regards the solution of the physical problem, the result of M. Gauss's theory may partly be judged from the concluding words of the Astronomer Royal's Treatise: "On the whole we must express our opinion that the general cause of the earth's magnetism still remains one of the mysteries of cosmical physics."

If, as I venture to hope, the missing links, not known to have been at hand, have in the present work been so brought together as to present the means of removing for the future this reproach of science, the labour which I have expended on this subject will have a fruitful result, while the simplicity of the methods adopted may have the effect of somewhat opening the field of thought to many, to whom

it had been unnecessarily sealed in the formulæ of the higher mathematics, by more scientific writers.

(27.) Another important consequence follows from the general cause which has been indicated. The principal magnetic poles, being the poles of the axis of revolution of the nucleus, are the points of *no friction* between the nucleus and the main body of the earth, and, so far as temperature depends on the earth apart from the sun, they may therefore be expected to be, as they are practically found to be, the points of lowest temperature, or, as Sir David Brewster has expressed it, "the poles of cold." There may thus also be expected to result a certain general conformity between the isothermal lines, or lines of equal temperature, and the magnetic isoclinal or isodynamic lines; and such has been remarked to exist. It will follow from this that, as the magnetic pole is receding from us in its secular revolution, the temperature of the British isles should be now gradually becoming somewhat warmer, so far as depends on internal terrestrial causes, until the magnetic pole, in its secular revolution, shall have passed, by some considerable distance, the opposite point of the great circle of the meridian of London. For it is to be observed that the secular period of maximum cold in England depending on this cause would not be the time of no declination of the needle, about 1660, when the magnetic pole was between London and the north pole of the earth, though the cause was then most directly operating, but it would be some time after; on the same principle that the coldest time of the year was found in the polar regions to be in February, and the coldest time of the night is not midnight but between two and three o'clock in the morning, and the hottest time of the day not noon

one from two to three inches, and the fall take not when the moon is on the meridian, the cumulative effect of her action going on for some time after she has passed the meridian. In like manner the maximum heat in England is far as *England*; as this amount of heat should have been reached probably towards the middle of last century. It is in accordance with this that Mr. Glaisher, of the Royal Observatory, Greenwich, in careful examination and comparison of the long series of meteorological observations taken there, says, in summing up his investigations, "The results are very remarkable. Large and continuous increases of temperature are shown in the months of November, December, and January." And he shows that the mean temperature of the year at Greenwich is now 2° warmer than it was 100 years back, and that the mean heat in January in the same period has increased 6°.

24. In the calculations which I have given, I would be understood not as binding myself to the

* In more easterly latitudes the time would of course be proportionally earlier. In 1709 the Asiatic Sea was frozen over. In 1726 the winter was so intense that people travelled in sledges across the strait from Copenhagen to the province of Scania in Sweden. In 1740 the snow lay eight and ten feet thick in Spain and Portugal; the Zuyder Sea was frozen over. At Leyden the thermometer fell 10° below the zero of Fahrenheit's scale, or 42° below the freezing point. All the lakes in England were frozen, and a whole ox was roasted on the Thames. The winters of 1744 and the five succeeding years were all extremely cold, as were also 1754 and 1755, in the latter of which the thermometer at Paris sank to the beginning of Fahrenheit's scale. The winters of 1766, 1767, and 1768, were intensely cold, as were also 1776, in which the Danube bore ice five feet thick below Vienna, and several other years up to the close of the century and slightly beyond. These facts, and many other interesting particulars, are given, Milner's "Gallery of Nature," pp. 512—515.

assumption of an exactly determined position or magnitude of the nucleus, but as giving a definite shape to reasoning which admits of being adapted to such modification of the details as the case may be found to require. In taking as the basis of my calculation the distance of the magnetic pole $18\frac{1}{2}^{\circ}$ from the north pole of the earth's axis, I have adopted a position as to latitude in agreement with that found from Capt. Parry's observations in 1819, which seems sufficiently supported, notwithstanding other somewhat varying facts, which is near the medium of the varying calculations, and which is certainly so far from being in excess as to polar distance, that it is $1^{\circ} 24' 43''$ *within* that observed by Sir James C. Ross, and half a degree less than Hansteen's ultimate allocation of it, while it corresponds also to certain astronomical facts and a consistent theory.

I have assumed the south pole of the nucleus of the earth, if produced to the surface, to be diametrically opposite the north. Yet this may not be exactly so as regards measurement, because, in the diurnal revolution both of the globe and of the nucleus, it is the centre and axis of gravity which have to be considered, and these may not exactly coincide with the centre and axis of measurement, and a small deviation at the depth of the pole of the nucleus might be considerable when produced to the surface. Yet it seems unlikely that the difference should be great.

(29.) There is an astronomical element which may enter into the solution of the problem. In Proposition III., page 5, it has been pointed out that the tilting of the moon's axis and equator in relation to her orbit may be considered a consequence of the change of the earth's axis, and of the shifting of the protuberant mass of the earth's equator from

the plane of the moon's orbit. The inclination of the moon's equator to her orbit is, as has already been stated, $6^{\circ} 39'$, while the inclination of her orbit to the plane of the earth's orbit, the ecliptic, is $5^{\circ} 9'$; so that the plane of the moon's equator leans $1^{\circ} 30'$ on the other side of the ecliptic. Now the question arises,—Is the amount of $6^{\circ} 39'$ in the tilting of the moon's equator what was to be expected as the effect of the tilting of the earth's equator by her change of axis $18\frac{1}{2}$ degrees?

The estimate of $18\frac{1}{2}^{\circ}$ here may not be quite exact, having been obtained from the inclination of the earth's equator to the ecliptic, by subtracting the inclination of the moon's orbit, neglecting the $9'$ (or $8' 49''$), which are over the 5° in that inclination: and if the inclination of the earth's equator to the ecliptic, which slowly varies, were taken at its present amount, $23^{\circ} 27' 23''$, the change of axis, on that supposition, would be $18^{\circ} 18' 23''$. But there is more probability of its having been above rather than below $18\frac{1}{2}^{\circ}$, since the obliquity of the ecliptic was probably above its present amount. That inclination has been slightly and slowly decreasing for the last two thousand years; and, as has been mentioned on the authority of the Astronomer Royal, in May B.C. 585 it was $23^{\circ} 45' 55''$. Whether, at the remoter period which we are now concerned with, it was less than that last amount, I cannot now say. But we shall take the estimate of the change of axis not beyond the mark, and sufficiently near for our purpose, at $18\frac{1}{2}^{\circ}$.

It will be borne in mind that the question we have now proposed to examine is one, not of the measure of the force of gravitation, which varies with the square of the distance, but of the angle or direction given

to the position, which is modified by the distance and not by its square. And, moreover, if, as most likely, the effect in question took place suddenly at one time, so it may practically be regarded as having taken place at one distance, though some oscillation might possibly follow for some time, gradually subsiding.

Now the elements which enter into the determination of this question are the following:—The moon's distance in semi-diameters of the earth; the earth's radius; the moon's radius (each in miles, according to the equatorial diameter, which is the greatest); the relative proportion of the weight of the moon to the weight of the earth; and the number of degrees which the earth's axis was shifted.

The two first of these, if multiplied together, produce simply the moon's distance in miles; and that distance varies from 251,940 miles at the farthest, to 225,720 at the nearest; the mean distance being 238,828 miles. But, within these limits, we are at liberty to assume the distance to have been greater or less than the mean, as the case may require. The earth's equatorial semi-diameter is 3,963 miles. The moon's equatorial semi-diameter is 1082·5 miles. The proportion of her weight to that of the earth is, according to the Astronomer Royal, as 1 to 80; according to Mr. Adams, as 1 to 81·5⁵. Of these

⁵ The late Mr. Edmund Becket Denison, in his excellent little work, "Astronomy without Mathematics," says, "Newton, from imperfect measures of the tides, made the earth 40 times as heavy as the moon, and Laplace, 70: Mr. Airy called it 80 in 1856. Mr. Adams's figure is 81·5, making the moon ·0123 of the earth, an easy figure to remember. Sir J. Herschell lowers the moon to one 88th of the earth." A correction of the measurement of the sun's distance lately made by astronomers would, as

several particulars it will be observed that the distance of the moon would operate to diminish the number of degrees which she would be tilted by the earth, but the great proportional weight of the earth would, on the other hand, operate to increase it. The elements may therefore be combined in a compound proportion as follows :—

As the earth's radius (R), multiplied by the moon's distance measured in semi-diameters of the earth ($\frac{D}{R}$),

To the moon's radius (r), multiplied by the inverse proportion of her weight to that of the earth ($\frac{W}{E}$),

So are the number of degrees which the earth was tilted in her change of axis (say $18\frac{1}{2}^\circ$),

To the number of degrees which the moon would be tilted in consequence (x).

$$(R \times \frac{D}{R} : r \times \frac{W}{E} :: 18\frac{1}{2} : x).$$

Now the product in the first term is plainly just D , the moon's distance in miles (if the radius be measured by miles), and this therefore will be the first term of the proportion. The distance may be taken at the mean, adding or subtracting so much, within the range of her eccentricity, as, on trial, shall suffice to give the actual amount of the inclination of the moon's equator.

Mr. Denison points out, according to one method of measurement by reference to the planet Venus, increase the estimate of the moon's weight, from Mr. Airy's figure of 80 to about 83; and by another independent method, he makes it 82.8. (Pp. 120, 228, 230.) It is obvious that the proportions to be now stated would be modified in their results by a small difference in this estimate of the moon's weight, or of the inclination of her axis, so that the results must be taken only as approximate.

The following proportions give the answer in general accordance with the fact, within limits depending on the future more perfect determination of part of the data :—

1. Assuming the earth's change of axis to have been $18\frac{1}{2}^\circ$, then on Mr. Adams's estimate of the moon's weight compared with the earth, namely, as 1 to 81.5, I find on trial that we must take the moon's distance at the time to have been about 6606 miles beyond the mean distance of 238,828 miles; and the proportion will stand thus :—

Moon's Distance.	Moon's Radius,	Inverse pro- portion of her weight.	Change of Earth's axis.	Effect on Moon's axis.
245,434	: 1082.5	$\times \frac{81.5}{1}$	:: $18\frac{1}{2}^\circ$	: $6^\circ 39' 0.048''$

Or, inverting the operation, to find the moon's distance at the time, from the degree of her tilting.

$$6^\circ 39' : 18^\circ 30' :: 1082.5 \times \frac{81.5}{1} (= 88223.75) : 245,434.49$$

Moon's distance.
Miles.

2. Substituting the Astronomer Royal's estimate of the moon's relative weight as given by him in 1856, which was as 1 to 80 of the earth's weight, then, taking the earth's change of axis as before at $18\frac{1}{2}^\circ$, the moon's distance at the time of the occurrence would be found by the same proportion to be 240,917.318 miles.

3. On Mr. Denison's estimate of the moon's proportionate weight, as 1 to 82.8, for which he shows very plausible reasons, arguing also that the Astronomer Royal's estimate in 1856 becomes now at least this, in consequence of the altered estimate of the sun's distance since adopted,—then, on the same estimate of the change of axis, $18\frac{1}{2}^\circ$, the same proportion would give the moon's distance at the time of the occurrence 249,349.4 miles.

All these measurements are beyond the moon's mean distance, but considerably short of her greatest distance, which, as already stated, is 251,940.

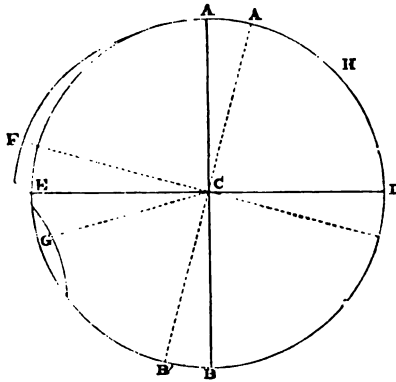
It is obvious that these results tend so far to confirm the view that the oblique position of the moon's axis has been caused by a change of the earth's axis, and are in agreement with the assumed amount of that change ($18\frac{1}{2}^{\circ}$) within moderate limits, as well as with the general reasoning in the preceding propositions; though they can be considered as yet only approximate, awaiting more accurate data in some particulars. Under certain supposable conditions they may also hereafter afford the means of testing some further conclusions, because the moon's distance is not arbitrary but dependent upon her position in her orbit, which stands connected with measures of time.

(30.) Before leaving this proposition, I desire to remind my readers that, however far the results are from preconceived opinions, the reasoning contained in it does not rest upon hypothesis. It is not hypothesis that the heaviest things fall lowest when free to adjust their relative position by their specific gravities. It is not hypothesis that the relative specific gravities of gold, quicksilver, iron, and other terrestrial substances, are as have been stated. It is not hypothesis that the density of the globe of the earth is double that of its superficial rocks, demonstrating that the substances in the interior have, especially towards the centre, largely arranged themselves in conformity with the above natural law. It is not hypothesis that a revolving globe of iron shows those magnetic effects upon the needle which Mr. Barlow's experiments have proved. Nor are the other magnetic facts which have been stated matter of hypo-

thesis. Neither is it hypothesis that the inclination of the moon's equator to her orbit is as has been stated ; or that it bears the stated proportion to the inclination of the earth's equator to the ecliptic. And so of other things. In short, the measure in which uncertainty enters into the conclusions which we have arrived at is much less than at first sight may have appeared.

PROPOSITION XVII.

IN the rotation of a slightly oblate spheroid (as $ABDE$) having a fixed centre (C), and free movement in other respects, if a considerable weight be added to the one side (as at F) at an oblique angle to the equatorial line of the rotation (ED), the effect will be to change the equatorial line, bringing it towards F by the centrifugal force,



and consequently to change the axis of rotation, till the balance is adjusted; as from AB to $A'B'$.

If on the other hand a portion of the weight be removed, as at G , or its momentum be diminished by bringing it nearer to C , the effect as regards the point G will be the opposite, that is, the equatorial

line will recede farther from G , till the balance is readjusted, and the pole A will recede with it.

If F and G be on the same side of the axis, but on opposite sides of the equatorial line, the axis will be similarly affected by the one as by the other.

Now the earth is so nearly a perfect sphere that it would be impossible to represent the small proportionate difference of the polar and equatorial diameters on the largest artificial globes which are made, which would be but as the thickness of the paper, and varnish with which they are covered. But the nearer the spheroid is to a perfect sphere the more sensibly, in proportion, will such an accession or subtraction of weight tell upon the position of the axis. For a perfect and exactly balanced sphere is indifferent to any position of its axis, and will change it on a slighter impulse, as any one may see who touches a spherical body floating in water. If the earth had been as oblate as the planet Jupiter or Saturn, a change of axis would have been more difficult to conceive and to account for. But the causes which produced the falling in of a great continent in the Pacific Ocean might by that occurrence itself affect the axis of rotation; for by that occurrence the weight was shifted nearer to the centre, as at G . Water, which occupied the vacant space only up to the sea level, is itself lighter than the rocks; there was therefore a very great shifting of the weight, and loss of its leverage power and momentum. Further, if the earth has a nucleus, either fluid or freely gravitating in a fluid, that nucleus did not require to be turned, in the change of axis, and its weight, (more than equal, probably, to a fourth part of the weight

of the whole earth)¹ is to be deducted from the mass of the globe in this consideration. It is further to be considered that the longer the arms of a balance, when nicely adjusted, the more sensitive it is. But the distance of the surface from the centre is the arm of the balance in the earth's equilibrium. The weight of the nucleus may reasonably be taken, as above, one fourth of the weight of the globe; its diameter at 3462·8 miles. While therefore a quarter of the earth's weight is thus to be subtracted, each arm of the balance is elongated by a line of 1731·4 miles interposed between the centre and the remaining weight of the earth on which the change would operate; and that weight being in exact equipoise on the two arms of

¹ The solid contents of the earth are given by Mr. Denison as equal to 259,845 millions of cubical miles (more exactly 259,845,072,639). The solid contents of a sphere are to those of the inclosing cube a little more than one half,—more accurately ·5236. Taking then that proportion of the cube of 3462·8 miles, it gives a sphere of 21,741,116,307·729 cubic miles as the nucleus of the earth, rather more than one-twelfth of the earth's bulk. Then taking one-third of this at the specific gravity of gold (19·358), one-third at the specific gravity of quicksilver (13·6), and one-third an equal mixture of the two, or their medium weight, the total weight of the nucleus would be 358,271,855,635 times a cubic mile of water. This is considerably more than a quarter of the weight of the globe of the earth, which at 5·5 for the mean specific gravity, would amount to 1,429,147,899,515 times a cubic mile of water. Lieut.-Col. James, from experiments with a plumb-line at Arthur's Seat, Calton Hill, and the Pentlands, calculated the earth's mean density to be 5·316. Another estimate makes it 5·4. The Astronomer Royal, from some experiments made in a coal-mine at Harton, calculated it to be as much as 6·566. The estimate adopted above (5·5), which was that of Newton and Laplace, may be sufficiently near for our purpose, and the allowance of weight made for the nucleus would leave the mean density of the rest of the earth still about 4½, while the surface rocks are only about 2½.

the balance, one half only is to be considered in regard to the proportionate effect of any weight added or subtracted on one side, because the opposite scale in equipoise contains one-half. It cannot be thought that the falling-in of a great continent in the Pacific Ocean on the south side of the old equator would be without some sensible effect upon the balance; it being equivalent to shifting the weight upon the one side materially nearer to the centre. Neither does it appear necessary to resort to Sir Charles Lyell's favourite stop-gap hypothesis of a few hundred thousand, or so many millions of years. If part of the subsidence be still going on, as he thinks to be indicated by the coral islands, that would no more show the whole to have taken place at the same rate, than the droppings from the eaves, an hour after a thunder storm, would give the measure and rate of the rainfall during the storm.

By what cause a sudden falling-in of a great continent in the Pacific might be produced, is not unlikely to be asked. That question does not fall strictly within the present abstract proposition; and it might suffice here to say that, assuming that sudden event to have taken place, it would tend, according to the present proposition, to shift back the north pole of the axis of rotation nearer to Europe: for, to take a rough general illustration, if, on the above diagram, the sunk continent were represented locally at *G*, Europe would be at *H*, or thereabouts. It may be convenient, however, to indicate briefly in this place the probable answer to that question.

Now, as has been pointed out, the first effects of a great meteoric fall of extra-telluric matter upon the earth would be balanced by an adjustment of the waters of the ocean; and for a considerable time, on

the occurrence of some successive meteoric showers, this process of adjustment would go on without disturbance, gradually accumulating a great weight of waters more than formerly in the South Pacific. But, as the proverb says, it is the last straw that breaks the camel's back. The weight of waters thus accumulated seem to have become equal to the utmost that the underlying strata could bear. One more dense meteoric November shower. One more rush of additional waters to the South Pacific, and the under strata of a vast tract there gave way, and fell in. A great continent there, on which the sea had, for some centuries, from time to time been rising, sank down, leaving only some mountain-tops above water, or near enough to the surface for coral insects to commence their operations on. The effect upon the earth's balance was immediate in changing the axis of revolution and shifting back the north pole, as has been described. There followed a deluge on the 17th day of the second month, that is, two days after the full moon of November, the month of the meteoric showers. How that flood necessarily resulted will be shown in its due place.

COROLLARY.—As the change of the earth's axis of rotation appears to have taken place in a way which was in conformity with the above proposition, it hence follows that there *was* a fixed centre; or, in other words, that the change took place around a central nucleus, which was not affected by it, but retained its old axis. For had the whole earth, centre and all, changed its axis, the movement of the pole would have been different. And had the mass of the earth remained unaffected, and only a superficial shell, of 25 miles thickness or so, changed its relative position in relation to the main body of

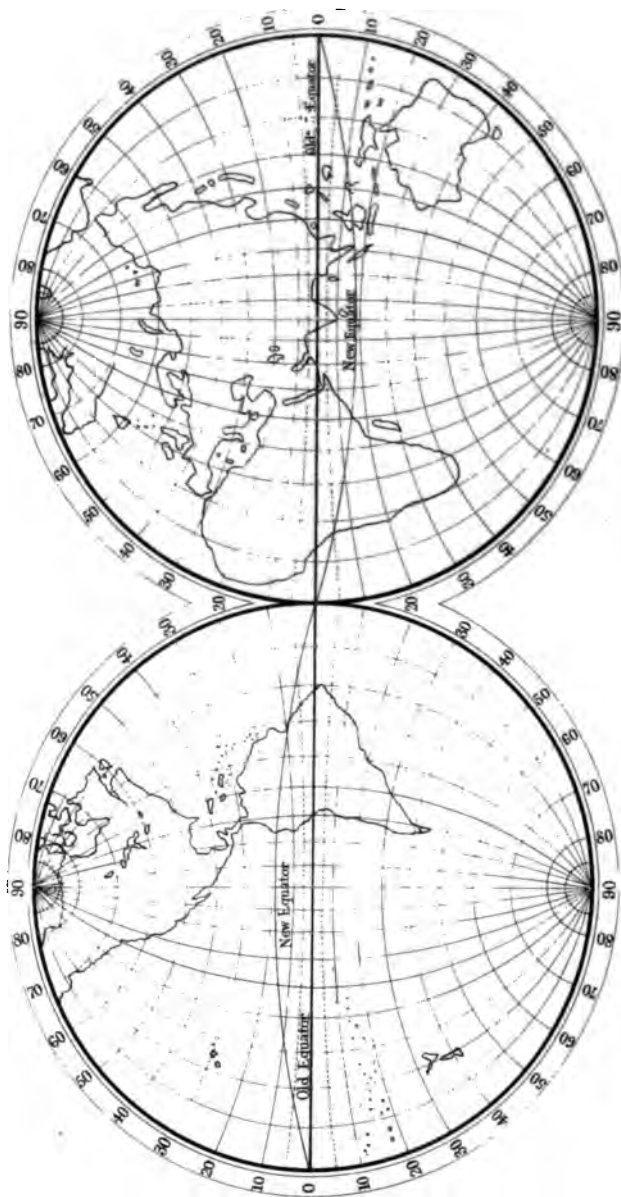
the earth and its axis, while the latter remained as before, the case then also would have been markedly different, because it would not have been a case of change of axis at all, but a change of superficial adaptation to the same axis. Such appears to be the case assumed in the hypothesis of Mr. Evans, alluded to by Sir Charles Lyell in the second volume of his "Principles of Geology" (p. 208).

PROPOSITION XVIII.

PROBLEM.—On the facts and principles above stated, to find approximately the probable position of the former equator of the earth's revolution, and to compare the effects which must demonstrably have resulted from the sudden change of the axis of rotation, with the known facts of geology, history, and experience, and particularly in regard to the deluge.

Without stating here in detail all the grounds which lead me to the selection, let it be assumed that the north pole was in latitude $71\frac{1}{2}^{\circ}$, and about longitude 105° west from Greenwich. The reasons for taking a latitude of $18\frac{1}{2}$ degrees distant from the present pole have already been given: the longitude must be somewhere on the opposite side from that of Siberia; and the one selected will, I think, be found to agree best with the direction of the continents and mountain-chains, bringing them more nearly into correspondence with the meridian, or into the transverse position in relation to it.

Take a terrestrial globe, elevate the north pole $71\frac{1}{2}^{\circ}$ above the north point of the horizon, and bring the longitude just given to the brazen meridian, so that the assumed position of the former north pole in latitude and longitude may be at the zenith of the globe, every where equidistant from the wooden



Map of the differing position of the Regions of the Earth
before the Change of Axis.

horizon. Then with a hair pencil, and a little lake or other water-colour, which may afterwards be removed without injury to the globe, describe a line round the globe, using the wooden horizon for the ruler. That circle will represent the former equator of the earth.

In order to assist the reader who may not have a globe at hand, I subjoin a small projection of the globe with that pole and equator, sufficiently accurate for our present general purpose; and have indicated on it also the altered position of the present or *new* equator, so as to assist the conception of the amount of change in reference to the various countries of the world.

The effects of such a position of the axis upon the climate of the various countries of the world will be manifest at one view. All the continents and islands of the old world, that is, of the eastern hemisphere, had a more southerly latitude than now. The continents and islands of the western hemisphere had a more northerly. Siberia was in a mild and genial climate, with a great open sea to the north from about latitude 60° , while the polar circle at that time was far removed from it, at latitude 85° . The torrid zone also was restricted on the other hand; the tropics being 5° on either side of the equator, as they are indicated in the dotted lines. The range of the temperate zone was much greater. The climate of India was more intensely hot. That of the West Indies, on the contrary, was much farther removed from the equator. The climate of Jamaica and of Vienna was nearly equalized, the former being only two degrees more southerly in latitude. The equinoctial line passed through the southern part of the Red Sea. The British Isles had a much warmer

climate. The Scandinavian peninsula was mild and genial. Iceland, Nova Zembla and Spitzbergen were temperate. Cape Horn and Tierra del Fuego were much warmer. New Zealand was warmer. Australia cooler and more equable. Hudson's Bay, though much more to the north, had its most northerly part five degrees from the more restricted polar circle. North America was probably more equable; cooler in summer, but with a winter less severe.

There is another class of effects to be next considered, arising from the oblate figure of the earth, and from the fact that the solid land would less readily adapt itself to the alteration in the position of the equatorial protuberance, than the waters of the sea. In those countries which were formerly in a considerable degree nearer the equator, the land then lay relatively lower than now: in those which were formerly farther from the equator, the land stood then relatively higher than now. To take an instance in the islands of the Eastern Archipelago, Borneo, Sumatra, Java, Celebes, and New Guinea lie ten or twenty degrees south of the old equinoctial line: but the present equator passes through the three former, and near the others. Australia also has been brought much nearer to the equator. The necessary effect is, that the change from the old to the new equator brought the sea-level much higher in relation to all these lands, and made them separate islands; whereas formerly the land stood relatively higher, so that, except where the sea exceeds in depth the amount of the rise of sea-level which resulted from the change, they were all united to the continent with the Malay peninsula, or with one another and Australia. In like manner, when, by the change of axis, the equator was brought

much nearer to the West India Islands, the sea there also rose relatively upon the land ; and it seems to me not improbable, on this ground, that the whole range of those islands, including the Caribbean Sea, may, in the former condition of the world, have been a continuous portion of the South American continent. On the other hand, upon the same principles, Europe, Egypt, Nubia, and Abyssinia, with the northern part of Africa, including the desert of Sahara, lay lower in relation to the level of the sea ; and by the change of axis the relative level of the land was raised. In the high latitudes the same effect would be more conspicuous in degree, and the north coast of Scandinavia might be expected particularly to exemplify it. The same would be the case in the rising of the coast-level of South America, because, by the change of axis, the equatorial protuberance of the ocean was removed farther from it.

Before proceeding to consider other demonstrable effects of the change, it may be as well to compare those already indicated with known facts, or with the recognized deductions from geological evidence. It has been pointed out that the latitude of Vienna and that of Jamaica were, prior to the change of axis, nearly in agreement, the former being only two degrees more northerly. And what is the evidence of geology upon this very marked point. "If we turn," says Sir Charles Lyell¹, "to the Upper Miocene formations of the West Indies, those, for example, of Antigua, San Domingo, and Jamaica, we discover in them species of corals similar to those found in beds of the same age at Vienna, Bordeaux, and Turin, and some of which, as Dr. Duncan has shown, have a

¹ "Principles of Geology," vol. i. p. 201.

near affinity to species now living in the South Sea, Indian Ocean, and Red Sea. They lead irresistibly to the opinion that there was a much greater analogy in those ages than there is now between the temperature of the West Indies in lat. 18° and that of Europe in lat. 48° ."

"The Upper Miocene flora and fauna of the whole of Central Europe afford unmistakable evidence of a climate approaching that now only experienced in subtropical regions. In one of the newest deposits of this Upper Miocene formation, Professor Heer has detected, at Oeninghen, in Switzerland, the leaves, fruits, and sometimes flowers of about 500 species of plants, in which we find a near resemblance to the (present) flora of the Carolinas and other Southern States of the American Union. After selecting 483 of these species as capable of comparison, specifically or generically, with plants now living, he finds that 131 are such as might be referred to the temperate zone, 266 to a subtropical, and 85 to a tropical latitude. In the present state of the globe the island of Madeira presents the nearest approach to such a flora. The proportion of arborescent to the herbaceous plants is very great, and among the former the predominance of evergreens implies an absence of severe winter cold. A rich insect fauna, such as belongs to a warm climate, is also attested by the great number of the species of those genera which are most easily preservable in a fossil state. The reptiles which play so insignificant a part in the Pliocene fauna of Central and Northern Europe form a more conspicuous feature in those Miocene formations. At Oeninghen there are two tortoises and three species of salamanders, one of them more gigantic in size than the living species of Japan. Bones of the

monkey tribe are also met with in Miocene strata near the foot of the Pyrenees in France. Among them is a gibbon, or long-armed ape, equal in stature to man, and the femur of a large species of this family has been detected by Dr. Kaup in strata of the same age at Epplesheim, near Darmstadt²”.

Next let us take Iceland and Spitzbergen. Prior to the change of axis, our map shows the former in latitude 60°, the latter between 63° and 66°, with an open sea around both, and the arctic circle removed back to lat. 85°. “The lower Miocene flora,” says Sir Charles, “has been traced from Italy northwards to Devonshire, and even to Iceland. In these high latitudes, however, the tropical and subtropical genera disappear, though the vine and tulip-tree, and some other forms, indicate a temperature 15° or 20° Fahr. warmer than that now belonging to the same countries³.” Farther north in Spitzbergen there is found a vigorous growth of fossil trees, taxodium, hazel, poplar, alder, beech, plane-tree, lime, and a potamogeton. “When the fossils are compared with the Miocene species of Central Europe and Italy, many of them are found to be the same, and it is clear that the climate was not only much warmer than now, but the temperature of Europe and the arctic circle was much less contrasted; nevertheless the flora of Spitzbergen was by no means so subtropical at the era alluded to as was that of Switzerland, Germany, and Devonshire⁴.” Greenland also bears in its fossil remains evidence of that more temperate climate which might be expected from the more restricted range of the arctic circle, and from a more southern latitude. Again, in the Isle of Wight and in the London clay

² “Principles of Geology,” vol. i. p. 200.

³ Ibid. p. 202.

⁴ Ibid. p. 203.

there are found during the Eocene period proofs of a climate in which the cocoa-nut, the screw pine, and the custard apple flourished⁵.

In regard to the alteration of the relative level of land and sea, in conformity with what has been pointed out as the natural and necessary consequence of the change of axis, and alteration of latitude, Sir Charles Lyell, in the first volume of his "Principles," has furnished us with a map of Europe, showing the extent of surface which had been covered by the sea at some time or other since the commencement of the Eocene period. That proportion is very great indeed, embracing a great part of Russia, Prussia, and Sweden, the whole of Denmark, and most of Holland, much of the Danubian Principalities, and the land between the Black Sea and the Caspian, which had been anciently connected in one sheet of water; besides the valley of the Rhone, the territory of Venice, and much of the lands along the Danube and the Po respectively, with part of Italy, Sicily, France, and a portion of the British Isles. These things are markedly in agreement with a lower level of Europe relatively to the sea. The fact is supported by numerous special instances. In England and Scotland abundant proofs are found in raised sea-beaches varying from 20 to 200 feet above the level of the sea. In Norway, at North Cape the rise has been 200 feet. Sir Charles Lyell says, "We discover sea-beaches in Norway 700 feet high, in which the shells are identical with those now living, although their geographical position has been somewhat altered, the fossil species constituting an assemblage which at present characterizes the sea several degrees

⁵ "Principles of Geology," vol. i. p. 204, 205.

farther north⁶;" but this last circumstance points to an origin of that deposit of shells different from that which we are now concerned with, in which the character of the shells would rather have been, like the general character of the Norwegian fossils, indicative of a more southerly climate.

Again, as respects the islands of the Eastern Archipelago, and the Australian continent, it has been pointed out that, in consequence of the change of

* "Principles of Geology," vol. i. p. 137.

Sir Charles supposes this hypothetical rise of 700 feet to have been gradually accomplished at the rate of five feet every century, and so assigns to it the imaginary lapse of 14,000 years! My friend, Dr. Boyd, has opportunely handed me a cutting from a number of the "New York Observer" of the present month (August, 1870), which, as it refers to the counter statement of a Norwegian geologist, I subjoin; and though Sir Charles Lyell's conjectural deduction from this particular alleged rise of 700 feet appears not to be quite accurately stated in it, no real injustice is done him in regard to the general length of the chronology which he assumes:—

"VERY POOR GEOLOGY.—Some remarkable facts in confutation of Sir Charles Lyell's theories of the myriad ages of geological evolution are supplied by the investigations of Professor Kjerrulf, of Christiania, who is making the survey of the Norwegian coast for the Government. He has examined the raised beaches and terraces, and declares Sir C. Lyell's theory (which requires 240,000 years for their present elevation) to be utterly baseless. In the first place, he says the uppermost limit of the sea action is only one tenth as high as Lyell states, and consequently that this single correction would cut down his figures from 240,000 to 24,000 years. Secondly, he proves that the coast has not risen by a constant slow motion, but by a series of sudden elevations, separated by periods of perfect rest; and consequently that all calculations based on a supposed uniform rate are worthless, and the total time spent in the elevation may have been very short. Finally, he says, the idea that the coast is now rising is entirely erroneous, this being a stationary period. Mr. Lyell must try again."

axis, the sea must have risen to a higher level upon them. In accordance with this, evidence is found that Sumatra, Java, Bali, Borneo, and the Philippine Isles, once formed part of the great Indian continent, being joined to the Malay peninsula and Siam; and that Celebes, the Moluccas, Floris, Timor, New Guinea, together with Lombok and Sumbawu, were joined to Australia. A deep, but narrow strait separated Celebes from Borneo and Bali from Lombok. The animals which inhabit these regions have been thought to bear testimony to the fact by their zoological classes and affinities'. Sir Charles Lyell assigns the conversion of the continental parts into islands to some portion of the Pliocene epoch. Further, there is evidence that the valley of the Nile was once an arm of the sea⁸, the alluvial deposits of Egypt lying upon a bed of marine sand and shells⁹.

The isthmus of Suez was covered by the sea. "We find also," says Sir Charles Lyell, "that the Desert of the Sahara was under water between latitudes 20° and 30°, so that the eastern part of the Mediterranean communicated with that part of the ocean now bounded by the west coast of Africa¹." "The continent of South America from lat. 34° S. to Patagonia, appears also to have been upraised throughout its entire width, since the beginning of the post-tertiary period²." Whether the rise is to be referred chiefly to that period or not, a still greater rise along the western coast of South America has taken place,

¹ Lyell, "Principles," vol. ii. p. 346—353, illustrated there by a map. "Physical Geography of the Malay Archipelago," cited from "Journal of Geographical Society," 1864.

² Hugh Miller, "Testimony of the Rocks," p. 374.

³ Humboldt, "Cosmos," i. 298.

⁴ Lyell, "Principles," vol. i. p. 250.

⁵ Ibid.

accompanied with violent volcanic agency in earthquakes, and the upheaving, in whole or in part, of the vast chain of the Andes. And volcanic agency is still active, so that in 1822 a vast area of country and of sea coast, extending from the Andes over about 100,000 square miles, was suddenly raised from two to four feet on the coast, and from five to six or seven feet inland. But it is not this continuous, or repeated and volcanic rise with which we are here concerned, except in so far as the series of causes put in operation by the change of axis may be regarded as continuing unexhausted, even to this day, in their remote and indirect effects. The formation of the mountain-chains of the Alps, the Pyrennees, and the Andes, which is referred to the post-cretaceous period³, may have resulted in whole or in part from the sudden change of axis, as we shall afterwards point out; but these are facts of a different nature from the general alteration of the relative level of land and sea of which we are now speaking.

Again, it has been remarked that the change of axis brought the equinoctial line much nearer to the West Indies than formerly, and consequently raised the relative level of the sea there; and the evidence derived from coral atolls demonstrates permanent

³ The upheaving of the Pyrennees is thought by M. E. de Beaumont to have taken place between the deposit of the chalk and certain tertiary formations "as old as the plastic clay." (Lyell, "Principles," i. 125.) The elevation of the Andes was still more recent, according to the same authority, and was accompanied by the simultaneous outburst for the first time of 270 of the principal volcanoes now active. The agitation of the waters of the ocean caused by this convulsion probably occasioned that transient and general deluge which is noticed in the traditions of so many nations. (Ibid. p. 123.)

subsidence in the West Indies, as well as in the lands of the South Pacific.

It is not to be expected that an absolutely uniform result can be shown, where many causes have been for thousands of years in operation. If there is evidence that the sea formerly penetrated farther into the St. Lawrence⁴ than at present, that is the contrary of what would exemplify the general case under consideration, and must be otherwise accounted for by local causes of the upheaval of the land; just as, on the other hand, the sinking of the forest of Cromer, or of part of the estates of the Earl of Godwin, now the Goodwin Sands, must be ascribed to local causes, and does not affect the general fact of the rise of the land of Europe.

But there is another class of effects inseparable from the sudden change of axis, and which may have presented itself already to the minds of many. In the diurnal revolution of the globe an hour of time corresponds to fifteen degrees of longitude, and a degree of longitude at the equator is equal to $69\frac{1}{2}$ miles⁵, so that the inhabitants of a place at the equator are carried at the rate of about 1038 miles an hour in the diurnal revolution of the globe. But as we recede from the equator the measure of a degree of longitude diminishes, at first more gradually, but nearer to the poles more rapidly, till at the poles it

⁴ Lyell, "Principles," vol. i. p. 250.

⁵ It is commonly reckoned from the older estimates $69\frac{1}{2}$ miles. But the above measure is that which results on taking the equatorial diameter 7926.6 miles, according to Archdeacon Pratt, whose treatise Mr. Denison considers the most complete on the problem of the figure of the earth. Mr. Denison gives the equatorial circumference 24,907, but according to the above diameter it would be 24,902 miles.

becomes nothing. The first eighteen and a half degrees measured from the equator would make a difference of about fifty-four miles an hour less, in the rate of rotation at the surface; the next eighteen and a half would make a difference of about a hundred and fifty-four miles less at lat. 37° than at lat. $18\frac{1}{2}^{\circ}$. But the last eighteen and half degrees, from lat. $71\frac{1}{2}^{\circ}$ to lat. 90° , would make the difference between about 330 miles an hour and nothing. At two opposite points in the tropical regions, where the old equator crosses the new, there would be no change in the velocity, but only in the direction of revolution on the surface of the earth; but at intermediate points along the old line of the equator there would be more or less diminution of velocity of the fixed solid parts of the globe, while the waters, retaining for a time their old impetus, would outstrip the land, and produce a great eastward tide, beating violently upon the western shores, and overflowing, until they adjusted themselves to the altered position, and, yielding to the modified direction of the centrifugal force, accommodated their level to the new line of equatorial oblateness. The maximum difference of the velocity of rotation produced at any place upon either of the equatorial lines would be about 54 miles an hour, somewhat under the speed of an express train, which is the amount of difference between the 24th part of the circumference at the equator, and at the latitude $18\frac{1}{2}$ degrees to the north or south of it. And that *maximum* difference could happen only at two points on each line of equator, 90 degrees of longitude from the points where they intersect. At all intermediate points along both lines the difference would be less. It does not appear to me that, within $18\frac{1}{2}$ degrees of the old equator, the change of axis would necessarily

be productive of a universal or general overflowing of the lands. The rush of waters in excess of the velocity would be met by the contrary tendency in those which came in from a northern or southern latitude; a violent agitation and eddying of the waters, as in a whirlpool or where two seas meet, would result, but I think it not improbable that a breadth more than equal to that of the old tropics, that is, a belt of from 10 to $18\frac{1}{2}$ degrees in width, was not affected in its highest lands with any complete deluge. That torrid part of the earth, however, where the range of the sun's vertical heat was then more confined and unremittent, had been considered, and probably was, altogether uninhabitable by man; and except as regards the tropical beasts, reptiles, and birds, the question regarding it is one not affecting the human race.

But in the temperate and polar regions the case was widely different. In the polar regions, indeed, there are two places, one on either side, each at the apex of an equilateral triangle having the line between the old and new pole for its base, at which there would be no considerable change in the rate of revolution, but only in the direction; and the same would be the case with all places in a line drawn on either side from the apex of the triangle to the intersection of the old and new equator. One of these lines would pass through Greenland, which would thus not be uniformly affected with a rise or subsidence of level*. But this would not prevent these places, or that line, from participating in the other general effects of the sudden change of axis. A degree of longitude in the parallel of $71\frac{1}{2}^{\circ}$ latitude,

* Lyell, "Principles," vol. ii. 236.

is equal to about 22 miles, and all things upon the surface of the earth at that latitude would be carried round in the rotation of the earth at the rate of about 330 miles an hour, or $5\frac{1}{2}$ miles a minute, about half the speed of a cannon-ball. But by the transference of a place at that latitude to the pole, where the rotation of the earth is nothing, while the impulse of that velocity continued upon all things, they would tend to continue in their onward course unless where firmly fixed and held to their position. The waters of the sea therefore would there be thrown violently over the land, and spirally round the world to the east, with an inclination from the north pole southwards. Pinnacles of projecting rocks would be snapped asunder by the impetus, and portions thrown forward upon the polar ice. For it must be next observed that the waters and the ice of the old pole, not having acquired the velocity of its new latitude, would tend to be left behind stationary, which would produce all the effect of an apparent motion contrary to the rotatory motion of the earth, with an inclination to the southward away from the new pole, the other waters of the sea, before mentioned, would violently meet this contrary mass of water and polar ice, and would lash over it, casting up sand, stones, shells, and fragments of rock upon the surface of the drifting floe, which probably might already bear from its original site rocks and pinnacles of cliffs which had been projected over it by the sudden impulse of the change. Great icebergs and vast polar fields of ice hundreds of feet thick, drifting towards the south-west in a spiral line, would become intercepted by the Liacov islands opposite the mouth of the Lena; and becoming stranded upon the shallows, or carried up upon the low lands

of the Siberian coast, would have a deposit of sand, clay, erratic blocks and boulders, or broken rocks, partly brought from their former site, partly lashed over them by the contrary rush of the other waters from the west. By the presence of such immense fields of ice, the floating drift of some portion of the neighbouring waters would be cooled and frozen to the mass; and both the frozen mammoth of the Lena, and the thick ice underlying a stratum of soil of but a foot or so, are thus accounted for. Detached masses and broken fields of ice, bearing on their surface many erratic blocks and boulders, would be scattered over a rushing deluge of turbid waters bearing them south-east or southward, and carrying along with them polar shells, sand, pebbles, and dissolved clay and soil, which, as the waters surmounted the face of lofty rocks and precipices, would be deposited more steeply and abruptly on the north-east or north, which was the nearer side meeting the rush, and with a longer and more gradual sediment on the south-west or south. The innumerable detached masses of rock and huge boulders scattered over Europe would be deposited as the ice melted or occasionally toppled over in a milder climate. That these erratic blocks are not found in tropical countries, and that they are found at least ten degrees farther to the south in the North American continent than in the eastern hemisphere, is what we should expect from the relative circumstances of the two cases. Further, it has been shown by Dr. Carpenter as one remarkable point ascertained in his deep-sea researches, that there are lower currents differing entirely from the upper, and adapting themselves to the configuration of the deeper valleys at the bottom of the sea. It was natural that the deep

current following the course of a natural valley should bring down and deposit only the drift of the upper country, fresh-water remains and pebbles of the well-known hills; while the sweep of a surface-current passed across above, and deposited here and there boulders and huge fragments of rock from its floating ice. This, as Sir John Lubbock remarks⁷, without rightly divining the cause, is the case in the valley of the Somme, in which the drift contains only the deposits of the upper country, with here and there an inexplicable boulder for which it is necessary, as he supposes, to invent a glacial epoch, and hundreds of thousands of years! There he finds bones lying together in the same deposit, and puts ten thousand hypothetical years between them⁸. And there he finds the flints of the river-drift "white and porcellaneous," "beautifully glossy," and "polished," "totally unlike any of those familiar to archæologists," or fabricated in later ages,¹ bearing, we may suppose, the prototypal character of the children of the sun and moon. But there are more things in heaven and earth than have been dreamed of in that philosophy. It is in accordance with all the principles on which this great and sudden event took place, that the great boulders and erratic blocks deposited by the drifting polar ice are not found towards the tropical regions, and that they are found about ten degrees farther south in North America than in the eastern hemisphere, seeing that America, previous to the change of axis, lay farther to the north, and Europe farther to the south than now, and that the sudden change might be expected to leave some trace

⁷ "Prehistoric Times," p. 358.

⁸ Ibid. p. 406.

^{*} Ibid. p. 342. 344.

¹ Ibid. p. 345. 370.

of that difference, in its effects on the course of the waters, and the deposit from the ice, in the two continents. If here and there an iceberg became stranded upon the mountains, and formed a permanent glacier in Alpine valleys where now there is none, or in Glen Roy in Scotland, forming lakes and parallel roads, and leaving other indications, the geologists may have in these places a glacial period without the absurdities of their present theories. The polar ice deposited by that diluvial rush of waters has not yet melted away in Siberia, and might remain for a long period in some parts of Switzerland. The drift of polar shells would similarly be accounted for, and we need no longer interpose hundreds of thousands of years between the drowned mammoth and the polar shells which sometimes characterize the spot. In the same way seeds of plants of Greenland or Spitzbergen would be carried to the Alps. Not only so, but lakes of sea-water would be left in many elevated situations, among the hollows and valleys of the mountains, in which marine shell-fish, corals, sea-weeds and sea-fish, then introduced there, might continue for many years to live and multiply, before those salt lakes dried up. And thus we may have some considerable marine formations at great altitudes without the necessity of putting those high Alpine levels down to the bottom of the sea.

Nor would so great a revolution confine itself to the surface of the earth. The tangential force, and the violent meeting of the waters of the opposite currents which have been indicated, would lash the waters into spray. The floods would clap their hands and mount into the winds in whirling water-spouts, with unwonted volumes of water, hurrying from the north towards the temperate regions.

There, as their tangential force and velocity became spent, the windows of heaven would be opened, and torrents would descend, while, on other hand, in consequence of retaining the centrifugal force acquired from the higher rate of rotation in the more southerly latitudes from which the transition had taken place, the underlying waters of the subterranean springs and channels would necessarily be thrown violently outwards and upwards to the surface, and thus "the fountains of the great deep would be broken up." The fluid and comparatively lighter quality of water renders it much more liable to this action of the centrifugal force than the more coherent and solidly massed substances of the earth, as we see exemplified in the water flying off the mop rapidly revolved upon the ship's side when a sailor cleans the decks. The graphic terms in which Scripture history records the effects are strictly and philosophically true, and are free from the speculative sophistry so eagerly swallowed as science by credulous infidels, worthy descendants of the alleged Darwinian ancestry. The mighty ramparts of the great mountain-chains, and especially in an interior circle, the range of the Caucasus, and Taurus, and Lebanon, and the Gordiæan and Armenian mountains, even though submerged, must have broken the violence of the diluvial rush of waters, and caused them, south-eastward of these barriers, to accumulate more quietly as in a comparatively sheltered basin, and there more gradually and slowly to subside.

It was stated by the Rev. J. Pye Smith, as an objection against the admission of a universal deluge, that there are trees now living in America which give proof of an antiquity greater than that of the flood of Noah, which could not have lived if submerged

for many months. But, even if we were free from doubt as to the age of those trees, it does not appear that there was any necessity for assuming that the rush of the diluvial waters continued so long over the whole world. A day under water would drown any number of the earth's inhabitants; but a tree might survive a considerably more prolonged submergence. The mode in which the event probably took place is borne witness to by the facts; and the physical causes, indicated to have been put in operation, would not produce a permanent deluge lasting for months over all the world, though it swept over and drowned all the world, submerging even the highest mountains, all except perhaps the uninhabited torrid zone, which included for a great part of its circumference the old and the new equator. In that part of the earth it appears not unlikely that the portions which were submerged by the change of axis did not include the highest lands, though passing in a rushing flood over considerably more than those portions of the ancient lands which, from the same cause, continue submerged still. The tropical elephant and rhinoceros, the hippopotamus, the lion, the tiger, the monkey, the crocodile, the boa-constrictor, the cobra, the ostrich, and innumerable beasts, birds and reptiles of that warm climate; and separated by a strait, the kangaroo, the emu, the flying fox, and other creatures of the Australian group, may have had that torrid region, which was then uninhabitable for man, as an ark of preservation for their several species. Hence the only surviving elephants are of the species destitute of hair. The other antediluvian sort, which belonged to the temperate regions, appears to have been larger; and indeed a larger growth and greater longevity may

have been generally characteristic of that period, when the climate was less variable.

We cannot doubt that the change of axis occurring, as it did, without alteration of the axis of the nucleus, was attended at first with much, nay, even violent internal friction, and generated much heat and electricity. Nor can it reasonably be questioned that great volcanic action was produced, and that mountains, or even mountain-chains, may have been upheaved by the same centrifugal force which acted on the subterranean waters. Trap and basalt and veins of fluid granite probably forced their way up through cracked and disrupted strata, over which they would spread themselves beneath the rushing deluge, at once slaking their fires and sending volumes of steam into the atmosphere. When the mighty flood subsided, the heavens looked down upon a new earth, and the earth looked up upon new heavens. The pole star of the past was gone; the constellations of the old zodiac were no longer in their old places: the sun and the moon were removed from their old path, both in their diurnal and their monthly and annual course. The ecliptic was not where it had been. The planets were strangely dislodged from their old habitats. And in the cloud man beheld a wondrous and magnificent token, not hitherto known to him, a pledge of the peace and mercy of his God. The change in the electric and magnetic condition of the world had, as we have shown², its effect upon the atmosphere also, and upon its glorious cloud scenery; nor is there reason for arbitrary qualification of the recorded fact, that the bow was then set in the cloud as it had never been before be-

² See above, pp. 134—136.

held, and was consecrated for a sign of the covenant between God and man for ever.

The present work has for its object the investigation of physical facts, not a theological review of them. But to the knowledge of facts and their mutual relations the testimony of faithful records is of inestimable importance; nor is there any greater proof of presumptuous weakness, than to disparage these, to sneer at those who pay regard to them, and to exalt in preference the ephemeral speculations of some infidel men of science, the moral fruits of whose reckless subversion of faith are now, and have been before, conspicuous in France. The child who has read in simple belief a narrative of the earthquake of Lisbon, or that of Calabria, knows more of the occurrence, than, apart from that direct information, a man of science could gather from the remaining vestiges, some of which he might perhaps incline to place a thousand or several thousand years ago. In the earthquake of Calabria, Sir Charles Lyell says, "Many persons were engulfed in deep fissures, especially the peasants flying across the open country, and their skeletons may perhaps be buried at various depths in the earth to this day^{*}." Apart from the *record* of the earthquake, what great and varying antiquity might be ascribed to these remains, if they should be discovered by geologists at some future time! And how much would be lost if the faithful records were superseded by pseudo-scientific groping in wilfully created darkness! So in regard to the deluge. The faithful transmission of an appalling and ever memorable fact, through a few generations in the family of the saved, down to the period of Abra-

^{*} Lyell, "Principles," vol. ii p. 140.

ham, is surely not difficult to be believed. In the period of that patriarch, in a book the original author of which is described as being "of the kindred of Ram⁴, and may not improbably have been AB-RAM, the prophet PATRIARCH of the families of that kindred⁵, there are some expressions which apparently contain allusions to the deluge, and which are exceedingly obscure unless so regarded. The prophet poet thus introduces the Lord as addressing Job. "Gird up now thy loins like a man; for I will demand of thee, and answer thou Me. . . . Who shut up the sea with doors, *when it brake forth as if it had issued out of the womb?* . . . Hast thou commanded the morning since thy days, *and caused the day-spring to know his place, that it might take hold of the ends of the earth*, that the wicked might be shaken out of it? It is turned as clay to the seal. . . . *Out of whose womb came the ice?* and the hoary frost of heaven who hath gendered it? The waters are hid as with a stone, and the face of the deep is frozen⁶."

There is here distinct allusion, 1. To the sea breaking forth in some prodigious manner, like the birth of a giant force, and being subsequently shut up as with bars and gates by the divine decree. 2. There is allusion to the alteration of the place of sunrise, "that it might take hold of the ends of the earth,"

⁴ Job xxxii. 2, and v. 15, 16. He speaks of himself in the first person, showing that he is the narrator.

⁵ "Ab" signifies "father;" hence "Ab-Ram," "Patriarch of Ram." The antiquity of the book of Job will be found vindicated and established, against the objections of some modern writers, in "Egypt's Record of Time," chap. xi., to which I refer, that I may avoid repetition here.

⁶ Attention was justly directed to this passage by Dr. Burnett.

which is the result of the change of the inclination of the equator to the ecliptic, the earth being “turned as clay to the seal,” and the place of sunrise taking hold of parts much to the north and south of the due east, or of its former range; and the moral end and issue of that alteration having been *that the wicked might be shaken out of the earth*: all which, together with the deluge, was the actual result of the change of the earth’s axis of rotation. 3. There is allusion to the glacial drift, vast masses of ice having made their sudden appearance as if they had issued out of some unknown and mysterious womb.

These allusions are the more remarkable because they are followed by a notice of the constellations of the zodiac, specifying, as I have elsewhere shown’, the four which were then in quadrature at the two equinoxes and two solstices. And these constellations stand thus in direct connexion with the subjects to which he had been alluding, the change of the annual course of the day-spring. There are allusions also in the same place to the electrical and barometrical state of the atmosphere, which we have found was affected by the magnetic consequences of the change. But, unless from their relation to the other particulars which have been specified, his expressions as to these might not be perceived to convey any definite reference to the present subject; as that God doth “make a *weight for the winds*, and *weigheth the waters by measure*, when He made a *decree for the rain*, and a path for the lightning of thunder.” These are not fanciful expressions; but, though the highest poetry, they embody thoughts which may make some moderns sensible that wisdom, even in their own

departments of science did not originate, nor yet will die with them. The same high state of ancient thought and science has also been demonstrably shown by the Astronomer Royal for Scotland in his works on the Great Pyramid⁸; from which it plainly appears that, in the age of its erection⁹, science was at a high stage of advancement, and men were certainly not nearer then to the orang-outang than some modern theorists who strangely wish to deduce their own pedigree from him, but are injurious to antiquity in making it so remote.

In full agreement with the sense in which we have understood these passages of the book of Job, St. Peter also appears to recognize the transmitted fact that the deluge was accompanied with a relative change of "the heavens" as well as "the earth" in their primeval antediluvian condition; then speaks of the deluge in which "the world being overflowed with water perished;" then of the post-diluvian world as "the heavens and the earth which now are," contradistinguished from those which had been, and from the "new heavens and new earth" for which we look hereafter¹.

In the record of the deluge preserved to us by Moses in the book of Genesis, it is the sacred narrative itself which we are to regard, without mixing with it the impression received in childhood from the works of toy-makers and wood-engravers. There is no mention of a peaceful procession of elephants, rhinoceroses, lions, tigers, crocodiles, hippopotami, zebras,

⁸ "Our Inheritance in the Great Pyramid," and "Life and Work at the Great Pyramid," by Professor C. Piazzi Smyth, Astronomer Royal for Scotland.

⁹ See Appendix D.

¹ 2 Pet. iii. 5. 7. 13.

and all the contents of the menageries, walking two and two into the ark. If Scripture be interpreted by comparison with Scripture, rather than by vain imaginations, the distinction between clean and unclean beasts would best be illustrated from Leviticus ; and it is understood, indeed, that these distinctions had been preserved from Noahic times. There is no mention there of the animals which have just been named. The book of Job seems to speak of Behemoth, Leviathan, and the unicorn (which are the elephant, the crocodile, and the rhinoceros), as well as of the lion and its cubs, as animals certainly not supposed to have "abided the crib," or to have had their "appetite stayed" by Noah and his sons. Still less does Scripture give us any hint that the cobra-capella, the boa-constrictor, and the rattlesnake, were included among the animals consigned to their care, or else we may be well assured they would have knocked them on the head². These wild creatures were doubtless preserved by God without Noah's intervention. The seal, the polar bear, and the walrus were not deprived of their floating ice, and some other creatures might partake of the same refuge. Some of the larger birds of prey might find a perch there on rocky fragments, and on trees thrown up upon them, or in other places on floating trees, and, though many must have perished, some might find occasional subsistence on floating carcasses. In the torrid equatorial regions another refuge has been already indicated for creatures of many other classes, in which at least a few of each might escape destruction. If there be

² Gen. iii. 15 ; Luke x. 19 ; Acts xxviii. 5. The two first of these passages have a figurative and symbolic meaning, but it has a literal base.

those who reject the thought of such secondary causes or means of preservation, and who would, contrary to nature, contrary to reason, and without countenance from Scripture, crowd all these animals into the ark, and convert it into a grand menagerie of wild beasts, let them go a step farther in the miracle, for consistency's sake. Let them admit that God can work without man's instrumentality; that he needed not man to save the lions and the tigers alive. He could do it in His own way, without the approved hypothesis of children and toy-makers. Let them leave it simply as a wonder of Divine providence, without dishonouring the scriptural narrative by adding to it a fashion of miracle not there recorded. He could save and did save some of these creatures alive: that is enough for those who make no account of second causes. But the Noahic list of clean and unclean animals makes no special mention of them, and seems primarily intended to apply in regard to those cases in which a question might arise as to their use and cultivation for human food. No question of that sort was likely to have arisen in regard to animals of the description which we have been speaking of; though the general distinctive marks given of the classes of clean and unclean, admit, no doubt, of being extended to them.

It is an unhappy circumstance that while many adhere devoutly to the early impressions of their childhood, without correcting them by Scripture more maturely and earnestly searched and considered, others, who in the pursuit of natural science have found some part of their early childish impressions untenable, nevertheless are in full agreement with the former class of persons in wishing to pin down Scripture language to mean only that which their

unreflecting childhood had understood to be its import; and this they do with a different purpose from the others, in order that they may represent Scripture as being at variance with science and philosophic truth. They produce to you passages in which Scripture mentions natural phenomena in natural, or sometimes poetical language, as of the sun's running his daily race as a giant; whereas, say they, this is contrary to science; but it has not struck them to look into their own scientific books and almanacks, and, when they find the same language there, of the sun *rising* and *setting*, and the sun's annual *course* in the ecliptic, and of the stars *rising*, and *southing*, and *setting*, to impute to that language when *there* employed, any contradiction of science and the Newtonian system of the world. Yet the language is the same; language which describes the phenomena as they present themselves, and which is not only the language of common life and common sense, but the necessary language of science itself. Natural phenomena, and the language which faithfully represents them, are at the basis of all science which is conversant with the *explanation* of the phenomena. And to remove the natural language which fitly describes the phenomena presented, would strike at the very root of science; for what can remain to science, if the very phenomena which it deals with, and the language which correctly states them, are accounted false? I submit, therefore, that the charge sometimes brought against Scripture of containing false science, because of its employing the language of phenomena, is brought either in gross ignorance of science, or with most blameable inconsideration, and is in itself most false and groundless.

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Again, when the week was instituted "for man³," it was appointed as a type of another and a greater week; its six working days of man, as a memorial of God's work of creation; its sabbatical day of rest, a type, as St. Paul informs us⁴, of "the rest which remaineth for the people of God." But that day of sabbatical rest is thus the revealed and the acknowledged type of a GREAT PERIOD. And thus we are furnished with a key, the analogical application of which not only *may be*, but, in all consistency, *must be* made. The six days of creation were GOD'S WORKING DAYS;—not man's, for man existed not, though man's week-days be made typical and commemorative of them;—AND "WITH GOD ONE DAY IS AS A THOUSAND YEARS, AND A THOUSAND YEARS AS ONE DAY." It is a period of vast and indefinite magnitude. Moreover, the record of the six days' creative work is preceded by a still more remote and general proposition wholly unlimited as to time, that "IN THE BEGINNING God created the heavens and the earth." Nor are we told how long the earth continued in its state as at first created, before it became without form and void, such as in that condition on which the six days' work operated to bring forth light and order and beauty. But we have scriptural reason to think that God did not at first create a dark chaos; for it is written that "God is not the Author of confusion," but "the Father of lights." These thoughts are, no doubt, beyond the child's first impression of the opening verses of the book of Genesis, to which it suits some would-be-scientific enemies of the truth to attempt to hold them down. But apart from the knowledge and the fear and love of the glorious

³ Mark ii. 27.

⁴ Heb. iv. 3, 4. 9.

Creator, natural science loses its value, and becomes eventually a powerful instrument of destruction. The power of reasoning itself becomes stultified and even inverted. The scantier the presumed evidence which they can adduce, the more extensive, strong, and audaciously confident their conclusions claim to be *upon that very ground*⁵. And an attempt to supersede faith in the divine work of creation, by substituting a spontaneous progressive development of men in millions of ages from baboons, and ultimately from fish or zoophytes, is not far from deserving the censure of the Psalmist, "The fool hath said in his heart, There is no God."

The historical record contained in the Scriptures of truth is, beyond the power of expression, of in-

⁵ Witness the following passage :—"Many of the most cultivated literary critics and some eminent mathematicians have shown, in the discussions which have arisen on the origin of species, an entire incapacity of weighing and appreciating the evidence for and against Transmutation, and this chiefly for two reasons : first, they have never been called upon, as classifiers in natural history, practically to decide whether certain forms, fossil or recent, should rank as species or as mere varieties—a point on which the most eminent zoologists and botanists *often disagree* ; secondly, *they are quite unconscious of the fragmentary nature of the record with which the geologist has to deal. To one who is not aware of the extreme imperfection of this record, the discovery of one or two missing links is a fact of small insignificance ; but to those who are thoroughly imbued with a deep sense of the defectiveness of the archives, each new form rescued from oblivion is an earnest of the former existence of HUNDREDS OF SPECIES, the greater part of which are irrecoverably lost.*" Lyell, "Principles," vol. ii. p. 484.

This is akin to the same writer's introducing prominently, as a head, in his classification of the PREJUDICES WHICH RETARD GEOLOGY, those which arise *from men not being fish*.—"Prejudices arising from our peculiar position as inhabitants of the land." Ibid. vol. i. p. 98. How does he propose to remedy that source of prejudice? By substituting hypothesis for proof.

estimable value. Faithful history is evidence of a kind with which nothing can compare. Men naturally lay hold even of faint historical vestiges where no distinct record can be had, nor should we be surprised to find even the faint and rude scratching of a mammoth upon a bone implement (though drawn *possibly* by some wag, by way of what he might conceive an innocent piece of deception to practise upon an enthusiast) accepted and gravely produced as evidence of being the work of a contemporary of the mammoth, and having been drawn from the life some hundred thousand years ago, if we were not struck with the supercilious weakness which could prefer such evidence to the record contained in the Scriptures of truth. The faithful scriptural narrative of the deluge contains in it the explanation of many facts which would otherwise be inexplicable and full of contradictions. And applying to it the chronology, which I have elsewhere carefully demonstrated even to the reconciling of the Hebrew and the Septuagint computation*, the exact day of the commencement of the deluge, which is recorded to have been the seventeenth day of the second month, accords with the views which have been expressed in the present work; for the patriarchal reckoning

* I am much pleased to find this reconciliation recognized by the Rev. Joseph Goodsir in his recent interesting and valuable work, "Seven Homilies on Ethnic Inspiration," (Williams and Norgate, 1871), as having been, as far as he can see, satisfactorily effected by me in my "Egypt's Record of Time to the Exodus of Israel." Mr. Goodsir's work now alluded to is a noble effort towards the furnishing of an "Evangelic Preparation," after the suggestion of Eusebius's great work of that name, by a careful and philosophic review of God's spiritual dealing with the Gentile nations of antiquity.

of the months, prior to the exodus from Egypt⁷, began with the first new moon after the autumnal equinox. The seventeenth day of their second month was therefore in our month of November, the exact day depending on the age of the moon. Now the deluge began, as I have proved according to the scriptural narrative, B.C. 3332, to which adding A.D. 1870, the interval to the same day of 1870 is $5202 - 1$, = 5201 years. (It is necessary, as the reader may probably be aware, in adding years before and after Christ, thus to deduct one year; because the termination of B.C. 1, was not a year before the commencement of A.D. 1, but coincided with it: and therefore reckoning from any given month of the one of these years to the same month of the other, the interval was not two years, but only one.) We may therefore be sufficiently near for our present purpose, to determine the age of the moon in the month of November in the year of the commencement of the deluge, in the following manner; by reckoning the number of lunations, and the number of tropical years, according to their exact measurement in days.

One tropical year = 365·24224 days⁸.

5,000 tropical years = 1,826,211·2

200 „ „ = 73,048·448

1 „ year = 365·24224

5,201 „ years = 1,899,624·89024 days.

⁷ Exod. xii. 2; xiii. 4. An indication of the previous reckoning of the beginning and ending of the year, still retained in some respects, may be found in Exod. xxiii. 16.

⁸ There are different estimates of the solar tropical year. That which I have given in the text is said by Mr. Denison to be the one generally settled among English astronomers. The length of the year itself is subject to a minute and slow change, and is com-

One lunation = 29·530588715 days.

60,000 lunations	=	1,771,835·3229
4,000 „	=	118,122·35486
300 „	=	8,859·1766145
20 „	=	590·6117743
7 „	=	206·714121005
64,327 lunations	=	1,899,614·180269805 days.

Lunations less than the period of years as above . . .	}	10·709970195 days,
		or 10 days, 16 hours, 48 minutes.

The new and full moon are therefore to be taken at the mean about 10 days, 16 hours, 48 minutes later in the month, for B.C. 3332, the year of the beginning of the deluge, than in 1870. Therefore, on a mean reckoning, the new moon corresponding to the new moon of September 25th, A.D. 1870 (at 6·34 morning, according to the Almanack), would fall on the 6th of October in the year B.C. 3332; which would thus be the beginning of the first month: the new moon corresponding to that of October 24, A.D. 1870,

puted by Lagrange to have been B.C. 3106, forty seconds longer. The nautical Almanack of 1828 assumed it = 365·242264 mean solar days. Sir John Herschell suggested the permanent adoption of the measure of 365·242216 mean solar days for the years after 1856. Sir Isaac Newton's estimate of the solar year was 365·24232638 days (or 365 days 5 hours 48 minutes 57 seconds). If this estimate were substituted in the above sum, it would make the number of days in 5201 years 1,899,625·33950238, and would thus make a difference in the excess over the lunations of about nine hours more. But the measure of a lunation was also slightly longer, there being, as is well known, a minute secular acceleration of the moon's revolution. No difference would be made at all affecting the broad purpose of the present computation, which is not a question of hours.

at 3.36 afternoon, would occur on November 4th, B.C. 3332, which date (or twelve hours later when the moon becomes visible) would thus be the first day of their second month; and the 17th day of the second month, B.C. 3332, would correspond to our 21st or 22nd of November, being just about seven days after the usual November fall of meteors, though not in the year corresponding to that in which their present maximum occurs. But the *maximum* shower at so remote a period might not be the same with the maximum now, but, by the copiousness of former falls, may have become much exhausted.

If the physical causes of the deluge have been connected with that meteoric class of phenomena, and the last addition to a series of heavy falls of cosmical matter had proved the immediate occasion of throwing towards the Pacific Ocean a weight of water at length exceeding, by the accumulated result of centuries, what could be sustained by the underlying strata, let it not be thought that that physical cause, so put in operation, militates against the direct and intelligent employment of it as an agent of the will of God, for the execution of His moral purpose. We know that the execution of His purpose had not only been long previously preparing, but had been even openly declared for a whole century before the deluge. In the familiar illustration which I made use of at the close of Proposition XIII., it would be absurd to argue that because the effect of the alarm was produced by a series of causes which might all be traced back to a silk thread slipping off the pointer of a watch, therefore the whole alarm was not to be ascribed to the will of my friend. In like manner the earthquake, with all its physical

relations, obeys the will of God, and may be employed by Him with purpose to knock off a fetter, and to start a lock, as easily as the Nasmyth hammer, ponderous as it is, may be employed to crack a nut; and the physical laws of nature are surely no more violated by the agency of God's will in the one case than by the agency of man in the other. Nature is violated only when God is denied and disobeyed.

It is worthy of special remark that the galvanic or electric element, which is proved to have so great a part in these operations, has also a peculiar and direct relation to the exercise of will; and that, in the constitution of man himself, that very element, acting in and through the system of the nerves, is the instrument of the subjection of the muscular movements to human volition, with that marvellous perfection, which removes even the consciousness of effort, and wields the bodily frame as if it were a thing not of earth but of spirit. SINCE, THEN, IT HAS BEEN PLACED BY THE CREATOR IN THAT RELATION TO THE WILL OF MAN, CAN IT BE THOUGHT THAT IT HAS NO APTITUDE OF SUBSERVIENCY TO THE WILL AND MORAL PURPOSE OF GOD?

I am far from wishing to decline any test which can be applied of the truth and correctness of the Scriptural record; and there is one which I have indicated in the latter part of the illustration of the sixteenth Proposition, which may here be briefly reverted to. It was there indicated that the moon's distance at the time of the occurrence was one of the elements involved in the question, and that, though the data are not at present sufficiently ascertained to enable us to determine that distance precisely, nevertheless there was a considerable probability that the moon must have been beyond her mean distance, but short of her greatest. Now, taking the Scriptural

date of the deluge at the entering of Noah into the ark to have been, as I have proved, Nov. 21 or 22, (say 22) B.C. 3332 (the 17th day of the second moon after the autumnal equinox), the interval thence to Nov. 22, A.D. 1870, estimated in *days*, is 1,899,624·89. But a revolution of the moon's apsides (that is, of the line which joins her least and greatest distance, takes place in eight years and 310·575 days; or, reducing the period all to days, 3,232·51292 days. Therefore 588 revolutions of the moon's apsides would occupy 1,900,717·59696 days, and measuring these back from the moon's perigee, which occurs on the 22nd November, 1870, it will be found that they carry back the time of the moon's perigee to the day corresponding to our 25th of November in the year B.C. 3335. But this is *before* the date of the deluge. Deducting the days which have elapsed since the commencement of the flood to the same day, Nov. 22, 1870, amounting to 1,899,624·89 days, the remainder is 1092·7 days, = 2 years and 362½ days, or three years all but three days. This, then, is the portion of the revolution of the apsides, which had taken place from the perigee next preceding the deluge, to the recorded day of the commencement of the deluge. The great inequalities of the moon's movements prevent us from arriving at quite an accurate determination from these measures; but those inequalities have their limits, and we may therefore already be in possession of facts sufficient to afford a confirmation of our own main position, and to prepare the way for the more accurate determination of the question by others.

Taking, then, the position of the moon's apsides on the 25th of November (or the day corresponding to it) B.C. 3335, to have been in correspondence with that of Nov. 22nd, 1870, being just 588 revolutions

previously, and setting out thus from the perigee of that day and year (B.C. 3335) then three days less than three years afterwards we come to the first day of the deluge, Nov. 22, B.C. 3332. What I wish to show is, that the moon was at that time beyond her mean distance, in conformity with the conclusions to which we were brought, and which are stated at the close of the illustration of the sixteenth Proposition; and this I propose to do by the following measures from the Nautical Almanack. For the interval above stated, from perigee Nov. 25, B.C. 3332, to Nov. 22, B.C. 3332, which was the commencement of the deluge, is the same as from perigee Nov. 10, 1864, at 6h., to Nov. 7, 1867, at the latter of which dates the moon was four days and twenty hours after apogee, or her greatest distance; and six days nineteen hours from perigee or her nearest distance. Again, the interval was of the same measure of time as from perigee Nov. 13, 1867, at 21h., to Nov. 10, 1870, which was one day and four hours after apogee, and eleven days eighteen hours from perigee. Again it was the same interval as from perigee, Dec. 6, 1864, at 10 h., to Dec. 3, 1867, when it was four days and ten hours after apogee, and nine days from perigee: and the same interval as from perigee Dec. 12, 1867 at 2h., to Dec. 9, 1870, four days after apogee, and eleven days from perigee. Again it was the same interval as from perigee, Oct. 13, 1864, to Oct. 10, 1867, five days after apogee, seven days and nine hours from perigee; or the same as from perigee Oct. 17, 1867, at 15h., to Oct. 14, 1870, two days and three quarters after apogee, and ten days from perigee. The inequalities in these cases do not invalidate the fact that in them all the moon was beyond her mean

distance; and that they all support the conclusion to which we were previously conducted on this head. But the accurate determination of the degree in which she was beyond her mean distance, remains for those to whom such questions appertain, and may be worthy of their attention.

I have now brought the treatment of this subject to the limit which I had proposed to myself; and I trust that it may result in the confirmation of the faith of many in the truth of the sacred Scriptures, and to the rebuke and turning back of some of the conceited and presumptuous, who flippantly retail at second-hand the infidelity of a few over-estimated men of science. Would God that it might awaken these last to some sense of their responsibility, and to a just review of their position, lest, in the coming day of final account, they be weighed in another balance than that of their own opinion or the praise of men, and be found wanting. It is not a matter to be trifled with by them, and certainly will not be trifled with by THE ETERNAL AND ALMIGHTY JUDGE, that they should permit themselves lightly to sap the faith of men in the Oracles of the Living God, or with a patronizing air condescend to approve them merely as the national epic, like Homer's poems to the Greeks. The Scriptures of truth are unlike all other books. They are the faithful and true record of the genuine revelations of God to the soul of man, with the historic relation of His dealings with man, in covenant, in law, in promise, in threatening, in chastisement, in justice, and in mercy; as well as of His mighty acts in creation, in redemption, in spiritual renewal from the ruin to which the fall had brought man's moral being, and in His good providence from generation to generation, carrying onward the grand

and glorious design towards the kingdom of His eternal Son in righteousness and peace. They have not been the product of one age, but from age to age, by His servants the prophets, have been received and faithfully transmitted; while from age to age they have been tested and tried to the uttermost, as no other book has ever been. They present the noblest effusions of inspired wisdom, the loftiest and purest sentiments and feelings of the heart of man, renewed and reconciled to God, and guided and empowered by His Holy Spirit; the brightest examples of holiness, of heavenly love, of devoted and unselfish magnanimity, and of the moral attributes of God manifested in and through human nature. The greatest intellects have pondered and searched them, and have recognized in them the very truth and word of God. The best and noblest hearts have found in them spiritual contact and communion with the Spirit of the living God. Childhood has received from them a simple and heavenly light; and manhood, in every advancing year, even to old age, has found that light still growing and brightening onwards to fuller and more perfect knowledge; even difficulties, when well and patiently examined, ever resulting only in fuller illustration and establishment. Let not the men of yesterday assume that the recent rapid advance in material arts and sciences is to explode the faithful inspired records of God's word, and to shake the foundations of truth, which four thousand years of trial have only been more and more firmly consolidating, though tested by severer ordeals than the most polished modern scorers can apply. We grudge not such the praise which they may possibly have earned in physical investigation, but let them prepare to stand with us at the bar of God's judgment. What award

they shall receive at His hand we presume not to forestall. The impartial records of Scripture do not withhold, even from the race of Cain, the credit of some important scientific discoveries and inventions, and improvements in arts; nor may it nullify some modern scientific reputations to consider of how many the faith has been recklessly tripped up, and how many souls have been ruined through their presumptuous rashness; but the day will declare it. It has been my endeavour by fair investigation in their own walks of scientific truth, to remove the stumbling-blocks which they have placed in the way both of that truth itself and of faith in the historical statements of the inspired Scriptures. While deeply conscious of the difficulty of the work, and of my own deficiencies, I do not dissemble the conviction that what was proposed has been in effect accomplished. In so far as this has been done, to God be the praise, on whom I have humbly and diligently waited for light and guidance; and to Him I now prayerfully commit the issue of my labours: for these investigations seem to me to bear testimony of His everlasting truth, and I behold mirrored and reflected, not in apocalyptic vision, but even in nature, the glorious Sun of Righteousness, encircled with the rainbow round about His head, which was set for the token of restored peace and blessing to this earth, and of the perpetual covenant to His redeemed from generation to generation.

APPENDIX A.

LAPLACE'S NEBULAR THEORY, WITH PROFESSOR PLATEAU'S EXPERIMENTAL ILLUSTRATION.

LAPLACE'S hypothesis, alluded to in Propositions I. and V., was extended by him to the whole solar system, which he supposed to have had its original process of formation from a nebulous condition, in which the whole matter of all the planets was at first included, in an unconsolidated state, as part of the same with the great central sun, and revolving with it, but became separated by gradual contraction, rotating faster as it contracted, and throwing off successive rings, vast and attenuated, rotating round the same centre, each occupying the whole orbits of the respective planets, into which the matter of the successive rings gathered on the breaking up of the rings.

I have only partially applied the theory, namely, as far as it may illustrate facts regarding the secondary planets and meteoric rings; which are facts whether the theory be accepted or not. But it may interest some of my readers to be furnished with an ingenious experimental illustration of it in a miniature form, which was devised by Professor Plateau of Ghent, and of which I may be permitted to extract the following concise account:—

“ Divested of technical terms, the experiment was nearly as follows :—Placing a mixture of water and alcohol in a glass box, and therein a small quantity of olive oil of the density precisely equal to the mixture, we have in the latter *a liquid mass relieved from the operation of gravity*, and free to take the exterior form given by the forces which may act upon it. In point of fact, the oil, by virtue of the law of molecular attraction, instantly takes a globular form. A vertical axis being introduced through the box, with a small disc upon it, so arranged that its centre is coincident with the centre of the globe of oil, we turn the axis at a slow rate, and thus set the oil sphere in rotation. We then presently see the sphere *flatten at its poles and swell out at its equator*, and thus realize on a small scale an effect which is admitted to have taken place in the planets. The spherifying forces are of different natures, that of molecular attraction in the case of the oil, and of universal attraction in that of the planet; but the results are analogous, if not identical. Quickening the motion makes the figure more oblately spheroidal. When it comes to be so quick as two or three turns in a second, ‘the liquid sphere first takes rapidly its maximum of flattening, then becomes hollow above and below around the axis of rotation, stretching out continually in a horizontal direction, and finally, abandoning the disc, is *transformed into a perfectly regular ring*.’ At first this remains connected with the disc by a thin pellicle of oil, which, however, on the disc being stopped, breaks and disappears, and the ring then becomes completely disengaged. The only difference between this ring and that of Saturn is, that it is rounded instead of being flattened; but this is

accounted for by the learned professor in a satisfactory way.

“ A little after the stoppage of the rotatory motion of the disc, the ring of oil, losing its own motion, gathers once more into a sphere. If, however, a smaller disc be used, and its rotation continued after the separation of the ring, rotatory motion and centrifugal force will be generated in the alcoholic fluid, and the oil ring, thus prevented from returning into the globular form, divides itself into *several isolated masses, each of which immediately takes the globular form*. These are almost always seen to assume, at the instant of their formation, a movement of rotation upon themselves—a movement which constantly takes place *in the same direction as that of the ring*. Moreover, as the ring at its rupture had still a remainder of velocity, the spheres to which it has given birth tend to fly off at a tangent; but on the other hand, as the disc, turning in the alcoholic fluid, has impressed on this a movement of rotation, the spheres are especially carried along by this last movement and revolve some time around the disc. Those which revolve at the same time upon themselves, consequently then present the curious spectacle of planets revolving at the same time upon themselves and in their orbits. Finally, besides three or four large spheres into which the ring resolves itself, there are almost always produced one or two very small ones, which may thus be compared to satellites¹. ”

¹ Prof. Plateau, “ On the Phenomena presented by a free liquid mass withdrawn from the action of gravity. ”—Taylor’s Scientific Memoirs, Nov., 1844 ; cited in “ Vestiges of the Natural History of Creation,” pp. 15, 16.

Mr. Denison² justly observes that “the experiment is defective in neither being able to represent the attraction nor the contraction of the globe, on which the whole theory depends.” It is obvious also that the one or two very small spheres which appeared with the larger in the experiment when the ring broke, had in them nothing in the nature of satellites, as they did not revolve as attendants round the larger, but might rather be compared to the small asteroids.

In connexion with this subject of the revolution of rings, the speculations which have arisen from the consideration of Saturn’s rings have a direct bearing on some parts of our subject, and we may take the following illustrations from Mr. Denison:—“Laplace proved that it (Saturn’s ring) could not be in one solid flat piece, but must be at least divided into two; and the same kind of reasoning has made it necessary to carry the division still further. For the outer and inner parts of a ring of any thing like that width require different velocities and periods to preserve their equilibrium, according to well-known laws of motion³.

“It is worth while to notice a remarkable result of the law of gravitation on the separate members of a large and uniform mass of either stars or nebulous matter.” “The square of the time of revolution varies as the cube of the distance divided by the mass of the attracting sphere. But if the sphere is uniformly dense or full of stars or nebulous matter, the mass itself varies as the cube of its radius, or distance of the outer particles from the centre. Con-

² “Astronomy without Mathematics,” p. 248.

³ Ibid., p. 172.

sequently the time of revolution of all the particles is independent of their distance, and depends on nothing but their density or closeness together; and the time varies inversely, or the velocity of revolution varies directly, as the square root of that density. And it may easily be proved that this holds, not merely for the particles revolving in great circles, or in a plane through the centre of the sphere, but also for those revolving in small circles like parallels of latitude round a polar axis.

“So we have this remarkable result, that a sphere of matter equally dense throughout, whether its particles are free as in a fluid globe, or freer still as in a nebulous one, can all revolve together, with equilibrium between the centrifugal and attractive forces, without any part wanting to lag behind the rest as they would in a flat ring like Saturn’s. Then if the inner parts alone contract and become denser than the outer, they will revolve faster; but the outer parts will revolve at the same rate as before; for the mass of the sphere within remains the same, and attracts as if it were all condensed into its centre, though the density is no longer uniform. Consequently the outside of a nebulous globe densest in the middle must revolve slower than the inner parts, if equilibrium is to be preserved⁴.”

Again, “The time of rotation of a shrinking globe,” says Mr. Denison, “decreases as the square of the radius; because each particle obeys the law of conservation of areas. Therefore the day would shorten an hour if the earth contracted a 48th⁵.”

Now as the earth is known to be demonstrably not of equal density throughout, these principles are

⁴ Ibid., p. 252.

⁵ Ibid., p. 214.

necessary to be considered in regard to it, if it be not solid and rigid down to the centre, but contain in some part of its depth a fluid mass, or an interposed fluid layer all round, such as to admit of free motion between the central part and the less dense outward body. This has been adverted to above, pages 119, 177. It is also worthy to be here remarked that the *vis viva* of a rotating body, that is, its energy in resisting any change of motion, increases with the square of the velocity (Brooke's "Elements of Nat. Philos.," page 193); so that the more rapid revolution of the nucleus, in conformity with the above principle, would greatly increase its adherence to its own rotation, distinct from the altered axis and rotation of the earth.

APPENDIX B.

REPORTED CASE OF A METEORIC STONE CONTAINING ORGANIC
MATTER, WHICH WAS REFUSED CREDIT SIMPLY ON THAT
ACCOUNT.

THE following case from the "Standard" Newspaper may, if true, have a bearing on the questions suggested at pp. 77, 78. The first of the letters appeared in the "Standard" of July 1, 1871. It will be obvious to the reader that it simply opposes *theory* to testimony: and that, assuming the persons attesting to be *honest men*, the fact confutes the scientific prejudices of its investigators. If the stone *was found* in the animal, as stated, that settles the question; and we are indebted to Mr. Loseby and his friends for ascertaining that the statement was not *all* fabrication. I subjoin with it a not unreasonable letter of inquiry which it called forth; and an answer to that, from Mr. Loseby, less facetious and more dogmatic than before; *valeat quantum*. If Mr. Loseby and his friends know all the meteorolites that have ever fallen, and have examined all the miniature planetoids moving in space, and know for certain that there is nothing akin to mountain limestone in any of them, but that every thing is comprised within the limits they have set, *then*, no doubt, he may be entitled to post the butchers and others in this case

as cheats and liars, persons disposed to fabricate groundless and false stories. The *moral* aspect of the question is thus worthy of some attention as well as the physical. But in his last letter it is plain that his distinction of acute and obtuse angles, in the alternative manner he puts it (I have marked the passage by italics), is, as applied, not very competent or scientific, since the same direction of impact must make *both* an acute and an obtuse angle with the surface at the same time, unless it struck perpendicularly. His idea of an improbability of the skull and the skin being sufficient to turn the course of such a projectile without fracture, does not agree with what I have been informed of in the case of some gunshot wounds. His whole argument curiously contrasts with the bold hypothesis lately put forth from the Presidential Chair of the British Association by Sir William Thomson (whether in earnest, or grave irony), that even the living germs of all organic vegetable and animal development may have found their way to this earth from those meteoric sources. But as the investigators in the case before us give their views as a rule for the public, I quote them in full.

TO THE EDITOR OF THE "STANDARD."

SIR,—There appeared in your impression of the 22nd inst. an account of a meteoric stone having been found in a cow killed at Glenn, Lincolnshire, on the 20th of June, during a thunderstorm.

Knowing that stones of truly meteoric origin were extremely rare, it appeared desirable that the present case should be investigated, so that it might be well authenticated if true, or prevented being permanently added to the long list of popular errors if false, which might be stumbled over years hence.

Accordingly, Dr. Pearce, Mr. James Plante, Mr. F. T. Mott, Mr. W. Millican, and myself, residents of Leicester, undertook the task of sifting the evidence. First, there was the testimony of the

butcher who had found the stone whilst dressing the beast; next, the testimony of the borough assayer of meat, who had heard the story from the butcher, and believed it after verifying it by passing the stone through the hole in the skin shown to him, but who had, nevertheless, condemned the meat on its own merits; and there was besides other human testimony at hand. But after hearing it we turned to the stone itself for the true solution of the question, and we had faith that, stone though it were, it would truly reveal to us, as we patiently listened to its whisperings, the history of whence it came, though that history might extend through the lapse of unnumbered ages.

We first inspected the stone on the 22nd instant, and noticed that the colour externally was a slaty brown; the surface somewhat rough and without gloss; the shape nearly spherical, and one inch and 37-100ths in diameter; the weight in air 1 oz. 17 dwts. troy; and that there were two remarkable spots on nearly opposite sides, resembling in appearance the craters of the moon.

After noticing these particulars, the question was asked of the stone—What is your specific gravity? and with the aid of scales and weights and a basin of rain water at 60 deg. Fahr., the answer came 2·64.

Are you magnetic or attractable by the magnet? and the answer came, by the application of a finely-poised magnetic needle, No. Then you do not contain an appreciable amount of iron in a metallic or oxidized state; or nickel in the state it occurs in meteorites; your surface has not the true metallic-looking crust; your colour is slaty-brown instead of black; your form is nearly spherical, instead of an irregular shape, dotted with round asperities; you are too light in proportion to your size by fully one-third; and, in short, you are not a meteorite?—Certainly I am not a meteorite. The globe has been my first and only home, and I am no wanderer from beyond the outer orbit of Uranus or elsewhere.

Having passed out of my department, the interrogation was taken up by Dr. Pearce.—You do not appear to be a stone which has been formed naturally in the body of the animal; your colour is too dark; you are much too heavy in proportion to your size, and there is no indication externally that your structure is composed of the concentric lamina which stones of animal secretion exhibit.—Thou hast interpreted truly: I am not an animal stone.

Questions were then generally asked—What are you? What has been your position here? And what are these two spots on your surface?—Split me, and you will learn.

This advice was carried out on the 26th, after permission had been obtained from the owner of the stone (Captain Goodchild). A small hole was drilled some distance down, and a hardened steel wedge driven in until the parts separated, when, lo! the stone proclaimed itself, through the interpretation of Mr. Plante, to be a piece of mountain limestone; and on the sections being placed under a fine microscope by Dr. Pearce, the mystery of the spots was cleared up by their being revealed as the remains of organic life which had existed before the lime became stone; together with other forms of aquatic life which had passed away many geological ages before man first felt the warmth, or gazed on the splendour, of the noon-day sun.

Had any doubt remained about the stone not being meteoric, these signs of organized life would have dispelled it; for although fragments of other worlds have occasionally reached the earth, they have not brought with them any signs of organized life.

As there appears to be a widely-spread erroneous belief of meteoric stones being comparatively common, perhaps the publication of this account may serve to correct it, and enable many persons to distinguish and preserve any true meteorite which may chance to come under their observation.

Yours, &c.,

E. T. LOSEBY.

Friar-lane, Leicester, June 28.

TO THE EDITOR OF THE "STANDARD."

SIR,—The letter of "E. T. Loseby" in your paper of this day is exceedingly amusing, but would be more satisfactory and convincing if it was his intention, and he had succeeded in upsetting the statement of the stone being found in the cow's hide, and explained how it got from its pristine locality into the place whence it was removed by the butcher, whose credibility your correspondent does not, however, attempt to doubt in direct terms.

Mr. Loseby has said nothing to shake the belief in the facts as stated of the animal being killed, and the discovery of the stone, but he has simply produced a somewhat facetious essay to prove himself a thinking researcher into the hidden truths of geology.

For the sake of the butcher, the borough assayer, and the other human testimony, whose truth is doubted by implication, it is to

be hoped that Mr. Loseby will carry his inquiries and inferences from premisses a little farther, and favour the world with the result. Unfortunately the cow cannot assist him, or her evidence would have been nearly as good as that of the stone, she being the principal party concerned.

I presume the gentlemen named by Mr. L., who assisted him in the task of sifting the evidence, are all geologists, but I much question if the conclusions were unanimous. The inquiry must have been most interesting, and deserves recording.

I am, Sir, your obedient servant,

T. R. VARDEN, Clk.

The Cedars, Putney, July 1.

TO THE EDITOR OF THE "STANDARD."

SIR,—The objects sought in the inquiry we undertook, and in the subsequent publication of the results, were: 'To determine if the stone were meteoric or not; to afford information by which the public generally might recognize meteorites; to show persons who might be disposed to fabricate groundless stories that the stones themselves would proclaim their falsehood and frustrate such attempts; and, lastly, to convey this to the public in a form which would be pleasant as well as instructive reading.

We believed all these objects had been fulfilled in the first letter, published in your impression of the 1st inst., and that our mission had, therefore, properly ended. Still, as there appears to be a desire to know more about the matter, the following details are given as the most reliable we have been able to collect:—

The heifer was killed by lightning whilst under a tree, and the tree was also struck, and exhibited a mark of the lightning down the bark, and some ivy entwining it was stripped off. The beast was disposed of by the owner to a butcher and sent to Leicester within twenty-four hours of its death, and it was dressed in the slaughter-house of another butcher, and during the first stage of this process the stone is reported to have fallen out from some part unconnected with the stomach or intestines, and presented itself to observation for the first time. A hole was found in the head over one of the eyes, through which it was said the stone had penetrated and passed in a slanting direction between the skin and the

skull for a short distance. The original owner of the beast had no knowledge of a stone being in the animal, or of any wound by which it might have entered, or of any blood arising therefrom, until the circumstances were reported to him after the carcass had arrived at Leicester. The beast was seen by two veterinary surgeons, one of whom—Mr. Bailey—accompanied the assayer of meat, &c., and the other—Mr. Burley—examined it independently. They noticed the hole and tried the stone in it, and found it fitted as a pocket; observed the skull was not fractured, or the outer covering of the bone (the periosteum [*sic!*]) injured. They accepted the story told to them as true, and did not make any examination with the view of questioning it.

When our inquiry commenced, which was on the second day after the thunderstorm, we were informed that the head had been boiled and the skin sent away, and it appeared there was nothing left of the animal by which we could test the story told by the butchers. Looking at the circumstances narrated in a physical point alone, they appear to us highly improbable; because if a stone of an inch and a half in diameter had descended to the earth with the usual velocity of falling bodies, its momentum must have fractured the skull; and supposing its descent could have been greatly retarded by some mysterious electrical agency, *it would have rebounded without entering the skin, if it struck at an obtuse angle, or it would have glanced off or cut a furrow down the skin if it had struck at an acute angle.*¹ And it did not appear likely, under any circumstances, that a stone of this size could have cut a hole through the skin, raised it from the skull, obtruded itself beneath, passed an inch or two down and lodged there.

We think all has now been stated that can be required relative to the case; but if your correspondent, the Rev. T. R. Varden, wishes to pursue the inquiry still farther, and solve the question which he asks us to undertake—of tracing the stone through its various stages, from the time it occupied its pristine position in the rock up to its arrival in the cow's hide; and if he is, moreover, willing to come down to Leicester and devote himself to the inquiry, we will give him a start by communicating all the information in our possession, and wish him well through it, although we cannot expect our lives will be prolonged until he has finished the task.

Yours, &c.,

Leicester, July 7.

E. T. LOSEBY.

¹ For the italics, see p. 242.

APPENDIX C.

NOTE ON A MAGNETIC THEORY OF BIOT'S, AS ILLUSTRATED
BY SIR GEORGE BIDDELL AIRY.

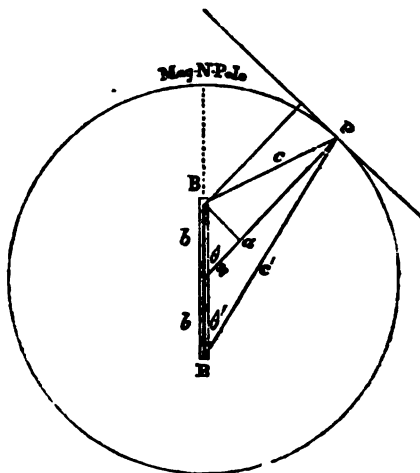
THE theory of Biot, referred to at page 166, that, to reconcile the phenomena, the contrary poles, or directive forces of the great terrestrial magnet, must be assumed infinitely near to one another and to the centre of the earth, is put by Sir George B. Airy succinctly for Cambridge mathematical students, presupposing a knowledge of some of the subordinate steps, which are unexpressed. It may therefore be of advantage to some readers, who may miss these, to point out that Sir George B. Airy's statement of it in his Treatise on Magnetism (page 103), depends on the XIIth and XIIIth propositions of the Second Book of Euclid, according to which in any triangle, (denoting the three sides by a , b , and c), if c be the side opposite the acute angle θ , then

$$c^2 = a^2 + b^2 - 2a \cdot b \cos \theta:$$

($b \cos \theta$ being the trigonometrical expression for the line intercepted between θ and the perpendicular upon a from the opposite angle). But if c (as c' in the following diagram) be the side opposite to an obtuse angle, then, supposing the perpendicular dropped from the acute angle θ' upon a produced,

$$c^2 = a^2 + b^2 + 2a \cdot b \cos \theta.$$

Now in the accompanying diagram, let B denote



the magnetic power of *one end* of the great central magnet of the earth, and let c be its distance from a point P on the surface of the earth: let the semi-length of the great magnet, to the centre, be called b ; let a be a radius of the earth drawn from the centre to P; and let it make with b at the centre the angle θ , the complement of the magnetic latitude of P. Then since the force with which the magnetic power acts is inversely as the square of the distance, therefore at P we have the force of that end of the magnet—say the blue end acting upon the red end of the needle—represented by

$$B \cdot \frac{1}{c^2} = B \cdot \frac{1}{a^2 + b^2 - 2a \cdot b \cos \theta}, \text{ otherwise written} \\ = B (a^2 + b^2 - 2ab \cos \theta)^{-1}$$

Its resolved portion in the horizontal plane at P is

found by drawing at that point a tangent to the circle of the magnetic meridian, and completing the parallelogram by drawing two lines from the opposite extremity of c ; the one line perpendicular upon a , and therefore parallel to the tangent; the other parallel to a , and meeting the tangent at right angles. The perpendicular thus dropped upon a , if measured relatively to b , is the *sine* of θ , and may be expressed $b \cdot \sin \theta$; it also cuts off from a a portion, x , towards the centre, which if measured relatively to b , is the *cosine* of θ , and may be therefore expressed $b \cdot \cos \theta$, which we shall frequently require to use. The former of these ($b \cdot \sin \theta$), is equal to the portion of the tangent representing the resolved horizontal part of the force at P, in the parallelogram of which c is the diameter in reference to which the tangential part is measured, expressed

thus, $\frac{b \cdot \sin \theta}{c}$. So that, combining it also with the expression for the reciprocal of the square of the distance, we have

$$\begin{aligned} & B \cdot \frac{b \sin \theta}{c} \cdot \frac{1}{c^2} \\ &= B \cdot \frac{b \sin \theta}{c^3} = B \cdot \frac{b \sin \theta}{(a^2 + b^2 - 2ab \cos \theta)^{\frac{3}{2}}} \\ &= B \cdot b \sin \theta (a^2 + b^2 - 2ab \cos \theta)^{-\frac{3}{2}}. \end{aligned}$$

The action of the opposite pole of the great magnet in the horizontal plane will be in like manner

$$B \cdot b \sin \theta (a^2 + b^2 + 2ab \cos \theta)^{-\frac{3}{2}}.$$

Assuming b to be infinitely small, (in which case θ and θ' have no appreciable difference), then the total horizontal force, which is the sum of these two

quantities, will (retaining, on this supposition, only the first term in the expression) be

$$2 B b \sin \theta . a^{-3}.$$

A similar expression with opposite sign gives the action on the opposite pole of the needle; and the algebraic difference, or numerical sum of these, gives the whole horizontal directive force

$$= 4 B b . a^{-3} \sin \theta.$$

Next in regard to the dip. The resolved part of the action of the first (the blue) pole of the great central magnet in the direction of the vertical at P, is

$$B . (a-x) . \frac{1}{c^3}$$

$$= B . (a - b \cos \theta) . (a^3 + b^3 - 2 a b \cos \theta)^{-1} :$$

which, expanded to the first power of b , gives

$$B . a^{-3} . (a + 2b \cos \theta).$$

The action of the other pole on the same end of the needle is

$$- B . a^{-3} . (a - 2 b \cos \theta).$$

The sum of these gives for the total vertical force downwards upon one end of the needle

$$4 B b . a^{-3} . \cos \theta.$$

As above, there is an opposite force, numerically additive upon the opposite end of the needle; and the whole vertical directive force is

$$8 B b . a^{-3} \cos \theta.$$

Hence the tangent of the dip at P

$$\begin{aligned} &= \frac{\text{vertical force}}{\text{horizontal force}} = \frac{8 B b . a^{-3} . \cos \theta}{4 B b . a^{-3} . \sin \theta} = 2 \cotan \theta. \\ &= 2 \tan. \text{ magnetic latitude of P.} \end{aligned}$$

And the total force at P =

$$\{ (\text{hor. force})^2 + (\text{vert. force})^2 \}^{\frac{1}{2}} \\ = 4Bb \cdot a^{-3} \cdot (\sin^2 \theta + 4 \cos^2 \theta)^{\frac{1}{2}}$$

At the magnetic equator, $\theta = 90^\circ$, and total force

$$= 4 B b \cdot a^{-3}.$$

At the magnetic pole $\theta = 0$, and total force

$$= 8 B b \cdot a^{-3}$$

or double that at the equator.

It is manifest that this reasoning, in leaving out of consideration all the terms of the series in which b is principally and increasingly concerned, implies that b is indefinitely small. In order to this, we must suppose c and c' to be brought indefinitely near to the radius a , and made practically to coincide with it, and the triangle to vanish at that limit; so that the angles θ and θ' are made virtually one and the same angle.

“It was found,” observes the Astronomer Royal, “that this elegant theory, though well representing the broad facts of terrestrial magnetism, failed in accuracy when applied to many special cases. Such curves, for instance, as those of equal dip could not possibly be explained by it. It was modified by supposing the axis of the magnet to be distant from the earth’s centre by one-seventh part of the earth’s radius; but it could not then be sufficiently reconciled with observations.” And after all, to what, as a physical theory, does it amount? Simply to reducing the supposed physical cause to a vanishing point. It is a mathematical abstraction analogous to a differential expression, but it is a physical nonentity and impossibility. As the expression of a limiting ratio, its somewhat rough and general agreement with the

facts appears to me to be adverse to other theories, and, so far, favourable to the view which has been presented above. But if c and c' be brought into one straight line coincident with radius a , what reason is there any longer why the dipping needle should be parallel to the tangent at the magnetic equator, and not equally so every where; there being, by this supposition, no difference any where in the distance of the two magnetic poles? (See above, p. 114, note.)

I may add that, as far as I see, there has not been shown ground sufficient for accepting the "conjectural" law that the tangent of the dip is equal to twice the tangent of the magnetic latitude. When the magnetic pole passed between London and the Pole Arctic, the magnetic latitude of London must have been about 70° , but the dip does not seem to have been much beyond (if beyond) $75^\circ 45'$, and the tangent of this angle would be to the tangent of 70° nearly as 1.43 to 1; certainly not as 2 to 1.

APPENDIX D.

ON THE DATE OF THE GREAT PYRAMID.

AT page 219 allusion has been made to the advanced state of science which existed in the age of the erection of the Great Pyramid. Now a scientific element, which must enter into the determination of the date of that pyramid's erection, has been found, with much probability, in a certain feature of the structure itself. The exact orientation of the Great Pyramid, that is, the placing of its four sides precisely facing the east, west, north, and south, must have involved exact observations of the stars and of the meridian, the plane of which is kept, not only in the position of the ground-plan north and south, but vertically also, the entrance-passage preserving exact conformity to it, within an error of $5'$ ¹; and it has been thought that this entrance-passage, which is on the north face of the pyramid, indicates, by the angle of its slope, that it was designed to point to the nearest recognized pole-star of that day, at its passage of the meridian just under the true pole. In consequence of the recession of the equinoctial points the present pole star was then remote from the true pole; and some other nearer star must have been

¹ Prof. Piazzi Smyth's "Life and Work at the Great Pyramid," vol. ii. p. 192; vol. iii. p. 109.

customarily observed instead. And if the slope of the entrance-passage of the pyramid is regarded as having at that time pointed to the star, the latter must have been distant from the true pole slightly more than $3^{\circ} 32'$, because the angle of the slope of the entrance-passage is, if any thing, rather more than $3^{\circ} 32'$ less than the latitude of the pyramid. Sir John Herschel fixed upon the star *α Draconis*, and computed by it that, according to the recession of the equinoxes, the time when its distance from the pole was such as to place it in correspondence with the slope of the passage, would be about B.C. 2400². This, however, was assuming the slope of the passage to be $26^{\circ} 41'$, and consequently the distance of the star from the pole to be $3^{\circ} 19'$. By accurate scientific measurement, Professor Piazzi Smyth, the Astronomer Royal for Scotland, has found that the slope of the entrance-passage is $26^{\circ} 27'$, or, by another measurement, $26^{\circ} 28'$ ³; and he determines the time

² In Col. Howard Vyse's second volume, pp. 107—109, cited by Prof. Piazzi Smyth, p. 57 of "Our Inheritance in the Great Pyramid," and also in "Life and Work at the Great Pyramid."

³ "Life and Work at the Great Pyramid," vol. i. pp. 167, 168, and vol. ii. p. 145, at the latter of which six measurements are given, all nearly agreeing. But in vol. iii. p. 249, of the same work, whether unconsciously or not, the Professor reverts to the measure which, on a balance of evidence, he had assumed as most probable for this angle before visiting Egypt, namely $26^{\circ} 18'$ ("Our Inheritance," &c., p. 58); and on this estimate of it he grounds his reckoning of the North Polar Distance of *α Draconis* at the time, namely, $3^{\circ} 42'$; which, if the angle of the entrance passage was $26^{\circ} 27'$, ought not to exceed $3^{\circ} 32'$, the latitude being $1' 12''$ less than 30° . Refraction would make a slight apparent difference in the altitude of the star and of the pole; a difference which, according to "Groombridge's Table of Mean Refractions," would, at $26^{\circ} 30'$ altitude, amount to an increase of $1' 56''$, and at 30° of altitude would amount to an increase of $1' 40.28''$.—See

when it pointed to the star α *Draconis* to have been B.C. 2170. The Professor points out the coincidence of an additional circumstance, that the Pleiades were at the same time upon the meridian to the south ; a coincidence which he deemed more noteworthy, since that constellation was a marked one in the notes of time taken by the ancients. But this is assuming the north polar distance of α *Draconis* to have been $3^{\circ} 42'$, which, by his own measurement of the angle of the entrance-passage, it would not be. The date which he thus considers to be indicated, would, according to my determination of the true scriptural and historical chronology, be in the lifetime of the Patriarch Abraham, forty years before the birth of Isaac.⁴

But this inference, as to the date of the Pyramid, is entirely based on the assumption that α *Draconis* was the star accepted as pole-star by the scientific men by whom the pyramid was designed and constructed ; though there was another star then nearer to the pole, of smaller magnitude, but visible to the naked eye. Also, though the supposed coincidence of the Pleiades being on the meridian at the same time might be worthy of remark, yet there is nothing *historically* to show that such *was* the case when the pyramid was constructed ; and the ancient regard paid to the Pleiades was rather in reference to the heliacal rising of that constellation, than to its midnight passage of the meridian. It appears to me, also, with the most sincere deference to Professor Piazzzi Smyth, that the general tendency of the facts which he has established, particularly in regard to

Prof. Barlow's Article on "Astronomy" in the *Encycl. Metropolitana*, vol. iii. p. 544.

⁴ "Egypt's Record of Time," pp. 369—401, and p. 483.

the sacred measures of the Hebrews being those employed in the pyramid, is in favour of a later date of erection, subsequent to Joseph's administration in Egypt. It has been shown in my work, "Egypt's Record of Time," that the historical and traditionary fragments of evidence which have been preserved concur in assigning it to a period later than Joseph. And I now venture to point out that the astronomical test derived from the slope of the entrance-passageway will agree perfectly with the later date, assuming the star then accepted as the pole-star to have been not *a Draconis*, but that nearer, though smaller star, which has been referred to above. It is a star of the sixth magnitude, visible to the naked eye under favourable circumstances, and probably more so in that clearer sky than in our northern misty atmosphere, situated nearly in the direct line between *a Draconis* and the present pole, distant from the former star about $4\frac{1}{2}$ degrees, almost on the same meridian, and about $20\frac{1}{2}$ degrees from the latter. It is this star which is entered in the British Association's Catalogue of the Stars as No. 4689, having, on January 1st, 1850, Right Ascension, 13h. 58m. 32.97s.; North Polar Distance, $20^{\circ} 35' 55.3''$. Not improbably that smaller star might be chosen by men of exact science for the sake of its nearness to the pole, rather than the bigger one for the mere sake of its brightness; for even though, for rougher practical purposes, mariners may have found it convenient to observe the brighter of the two, very different reasons might, in the erection of such a work as the pyramid, influence the man who was the highest man of science of his own and many following ages.

The precise date when that small star was at the distance of $3^{\circ} 32'$ from the pole will require accurate astronomical calculation. But a reasonable approximation may suffice for the present. Now, taking the means within reach, I have drawn a circle on my globe (one of Newton's fifteen-inch, corrected up to 1851) at $23\frac{1}{2}$ degrees distance, and another at 24 degrees distance, round the pole of the ecliptic. The gradual slow revolution of the pole of the earth's rotation, in correspondence with the recession of the equinoctial points, takes place now nearly in that first circle, or, with allowance for greater obliquity of the ecliptic at the remote past date, it would be within the second. The star *α Draconis* would be on the circle, or close to it, at an interval of about 65 degrees of recession, equal to about 4673 years before the close of A.D. 1850; that is, about B.C. 2823: and the same star would be at the distance of about $3\frac{1}{2}$ degrees from the pole, at an interval, measured back from the close of A.D. 1850, of 56 degrees, or 4024 years (= B.C. 2174), taking the period of the complete revolution of the equinoxes to be 25,868 years. The Astronomer Royal for Scotland has given an accurate calculation of the date in his "Life and Work at the Great Pyramid," and he makes it B.C. 2170; but he is taking the North Polar Distance of *α Draconis* at the time to have been $3^{\circ} 42'$. The near correspondence of my rough figures may serve to show that they produce a result at least reasonably approximate; and as the North Polar Distance of the star, on the authority of the Professor's own measurements of the angle of the Pyramid's entrance-passage, as above cited, should be $3^{\circ} 32'$ (10' less than he has counted it), this, if taken into account, would have the effect of somewhat adding to his estimate of the time.

Now applying the same method of measurement to the smaller star which was then nearer the pole, if we take the space of $3\frac{1}{2}$ degrees of a great circle from that star to the small circle in which the pole of the equator has been receding round the pole of the ecliptic, then, calculating from the nearer of the two points at which that measure would meet and intersect the circle, the result to which we are brought is the following: Supposing the obliquity of the ecliptic to have been $23\frac{1}{2}^{\circ}$, the intersection would have taken place at the distance of about $47\frac{1}{2}$ degrees of recession. But if we assume the decrease in the obliquity of the ecliptic to have been going on from that remote age at the same average rate as from May B.C. 585, when the obliquity is estimated by Sir George B. Airy to have been $23^{\circ} 45' 55.6''$, then, for our present purpose, at the remoter date it should be taken at least at $23^{\circ} 54'$; or, applying the same average annual rate of diminution of the obliquity given in the Nautical Almanac between the years 1864 and 1870, which is $= 0.476''$ a year, the obliquity, say in the year B.C. 1635, would be $23^{\circ} 55' 13''$. With this distance for the circle round the pole of the ecliptic, the measured space of $3\frac{1}{2}$ degrees from the star would intersect the circle at a remoter point, giving between 48 and 49 degrees of recession before A.D. 1850; I should say probably not more than $48\frac{1}{2}$ degrees, but certainly beyond $48\frac{1}{8}^{\circ}$. We should have then the following proportion, taking it at the former of the measures:—

Degrees.	Degrees.	Years.	Years.
360	: $48\frac{1}{2}$	:: 25,868	: 3485 (very nearly)
		Deduct	1850
		Date B.C.	<u>1635</u>

Now if we take the date of Sesostris' return from his warlike expeditions, according to my table⁶, calculated on a thorough investigation of all the chronological data, it was B.C. 1666, equal to 3516 years before the close of A.D. 1850. Inverting, therefore, the method of the proportion, we obtain the amount of the recession of the equinoxes since that date down to the close of 1850, as follows:—

25,868 yrs. : 3516 yrs. :: 360° : $48^{\circ} 55' 53.39''$,
recession since B.C. 1666.

But even if directions were issued, and measures taken, immediately on his return, much time must have been consumed in preparatory work, digging, quarrying and cutting the stones. Ten years, as Herodotus tells us he had been informed, had been occupied in making the road alone, a work not inferior, in his estimation, to the Pyramid. Then the laborious conveyance of the materials to the spot followed. Twenty years were then occupied in the erection of the Pyramid itself; and Herodotus estimates that the preparatory labours in stone-cutting and conveyance to the spot must have taken almost an equal time. Assuming, therefore, that immediate employment was thus found for the captives brought home from the warlike expeditions of Sesostris, and that the preparatory works were commenced on his return to Egypt, B.C. 1666, the time to which our computation from the angle of the entrance-passage has conducted us, namely, B.C. 1635, is one quite in agreement with the historian's estimate of the time consumed before the building ever began to rise above the level of the ground. Nay, supposing the amount of the recession down to 1850 to have been

* "Egypt's Record of Time," p. 371.

as low as $48\frac{1}{8}$ degrees, even in that extreme and less probable case, the date to which we should then be conducted (= B.C. 1609) would still fall within the lifetime of Sesostri the Great, who died B.C. 1607; and certainly a large proportion of the building had been executed before they reached the height of the entrance. But, as closely as I can judge, the measure of the recession was more nearly about $48\frac{1}{8}^{\circ}$; and, if so, the date of the sloping entrance-passage would be about B.C. 1627, twenty years before the death of Sesostri, and would correspond to a point of time about eight years after the commencement of the actual building of the Pyramid, and twelve years before its completion, by the data given above.

The coincidence is the more worthy of notice, because my former calculations, on which the tables were constructed, had nothing whatever to do with the present calculation based on the angle of the sloping entrance-passage of the Great Pyramid, and its relation to the pole-star of that day.

It may be remarked, that at the date now assumed, when this small star was on the meridian under the pole, there was at the same time on or close to the meridian southward, and on or very near the equinoctial line, the principal star of Orion, called *Menkib* *Al Gjauza* or *Beltegeux*, a star of the first magnitude. Here, then, was considerable help towards ascertaining an astronomical meridian line, in two points well defined, north and south, with a single star at each.

This approximate determination by the globe requires, of course, much more exact testing. But it may be sufficiently near to call forth the application of more accurate methods.

In conformity with the above conclusions, the fol-

lowing general reasons induce me to think that the erection of the Great Pyramid must have been later than the period of Joseph in Egypt:—

1st. The enormous expense and labour of the work imply enormous wealth at the disposal of the king; and that seems to have been the immediate result, in the first place, of the seven years of plenty and the seven of famine under the administration of Joseph, by which all the property and persons of the Egyptians, except of the priesthood, were brought into the possession of the monarch; and, in the next place, of the great expeditions, conquests, and plunder of the nations, to which Sesostris applied his vast resources, and by which he brought to Egypt the wealth of the world.

2ndly. If, as the late John Taylor, of Kensington, and Professor Piazzzi Smyth, on very plausible grounds have thought, the coffer of the Great Pyramid be, in point of capacity, the corn-measure, the same with that used by Joseph,—and that the name “pyramid” may be derived from *πυρός*, *corn*, appears from that derivation and explanation of it being given with others in the “*Etymologicum Magnum*,” from which Scapula also quotes it, saying “*πυραμίδες* apud Ægyptios etiam dicuntur regis granaria a Josepho ædificata”—then, since the sales of corn had assumed, under that patriarch’s administration, so great a political importance, the probability is increased that the measure would receive attention from the scientific men of the period next following: and such a man as Thoth, the Bacon and the Newton of his day, might be capable of appreciating Hebrew truth. By him, I think, the Pyramid was probably designed, and its building superintended, though he may not have lived to its completion.

3rdly. A reason is thus furnished for the sacred cubit of the Hebrews finding its way into the measurement of the greatest monument of Egypt.

4thly. Thoth, of all the men of Egypt, is the most competent and perfect man of science to whom the construction or design of the Great Pyramid could be assigned.

5thly. The tradition preserved by Herodotus expressly assigns the Pyramid to Kheops, who appears to be the same with Phiops or Aphiops, and by interchange of the asperated consonants Aithiops "the Ethiopian king." The first king of that dynasty which knew not Joseph, was the father of Sesostris, and was reigning at the time of Thoth's observation of Sothis; but Sesostris himself also had the designation of Aithiops, and is probably the Kheops, Phiops, or Aphiops, of the tradition.

6thly. The question of whether the coffer of the Great Pyramid was intended to be used as a sarcophagus for the king may reasonably be discussed, without militating against the fact of the coffer having been truly the corn-measure, since it might be even mystically so applied, in compliance with the teaching of the resurrection by the mysteries of CERES, and the regarding of man in death as a corn of wheat fallen into the ground. But there is no evidence that it ever was used as a tomb, and so great a structure indicates that, even if this application had been intended to be included, other more important ideas were attached to it. Although Professor Piazzi Smyth may, in some respects, have pressed to an extreme the scientific metrological indications of the Great Pyramid, yet it is done with true scientific genius; and, secure in the main facts, he may afford to bear some opposition and detraction.

In one respect the Professor's work reminds me of the Bridgewater Treatise of Sir Charles Bell on the Hand, who shows in the hand of man a relation to the magnitude and density of the earth, the depth of the atmosphere, the distance of the sun, and the constitution of the universe, as well as more minutely to the fins of fishes and the wings of birds. But if the metrology of the Great Pyramid be so perfect and so extensive in its relations to the globe of the earth, as the Professor endeavours to show, it would no doubt necessarily imply (as he contends that it would) some basis of revelation; and it cannot be denied that weight and measure have in various instances been made the subject of revealed directions.

7thly. The exact astronomical orientation of the Great Pyramid, and the meridian and polar reference of the entrance-passage, appear to me to be in a high degree agreeable to my suggestion ("Egypt's Record of Time," pp. 340, 341) of the theological reference of the Pyramid to Jacob's dream of the ascending scale of being, and to the divine Trinity as the heading up of all in One. I think so, because I have obtained abundant proof that the ascending scale of angelic, seraphic, and cherubic spiritual life, came to be viewed in association with the four quarters of the outer world, and that the ζωα, or living creatures of the zodiac, originated in that grand spiritual view of the living universe, the four faces of the cherub being placed in quadrature with the four cardinal points of the heavens. (See at large in my "Gate of Prophecy," vol. i. pp. 151—202, and "A Clergyman's Holidays," pp. 203—227.) The square pyramidal form may express a modified form of the same idea with the spherical "wheels" of Ezekiel; and Professor Piazzi Smyth has shown that

the Pyramid, in the proportions of its height and semi-circumference, contains in a marvellous manner the squaring of the circle. It is remarkable that the name *Βαιτύλια* "Bethels," was afterwards applied to pyramidal erections of stone, as Bochart shows; thus directly connecting their origin and meaning with the dream of the patriarch at Bethel. To sleep at the base or in the heart of that glorious pyramid of celestial natures, to awake to the consciousness of the living presence of God, as being in "none other than the house of God, and the gate of heaven," might suggest to some the emblematical application of that pyramidal form in association with the sleep of death, and the faith of a conscious awaking hereafter to the glories of heaven. Unquestionably, whether in the case of the Great Pyramid or not, that application to sepulture *was* made in some structures of that nature; but this does not show that the vast structure of the Great Pyramid meant nothing more than a tomb! Professor Piazzzi Smyth thinks that it did not even include that purpose; and he has certainly proved that other and totally different meanings were contained. We might apply to it, what is said by Philo⁷ of the cherubic emblems, that they expressed *ἐπίγνωσις καὶ ἐπιστήμη πολλή*, "much information and scientific knowledge," or what Clement of Alexandria tells us that their very name suggested, *δηλοῦν ἐπίγνωσιν πολλήν*, "to body forth much knowledge⁸."

It was not to be expected that the residence of the Patriarch Israel and his descendants in Egypt for so long a time, and at first in so influential a

⁷ De Mose, lib. ii.

⁸ Strom. v. Opp. fol. Lut. Par. 1641, p. 563.

position, could be without some effect in moulding or modifying the opinions of the age. And the freedom of the Great Pyramid from every idolatrous symbol gives a striking testimony of the impress of that pure faith, which at that time was peculiarly distinctive of Israel.

It appears to me, then, that the revelation made to Jacob at Bethel may have contained some exact details in harmony with the patriarchal cherubic doctrines, and which probably were preserved by the patriarchs, and afterwards, commanding admiration from their grandeur, became embodied in the Pyramid. Nor is it to be wondered at if the most durable masonry acknowledges some relation to the ladder, or steps, of that celestial structure, on which the angels of God were beheld by the patriarch ascending and descending, and the Lord at the one glorious apex into which all things are headed up.

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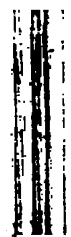
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